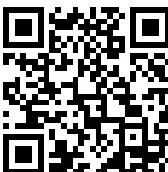


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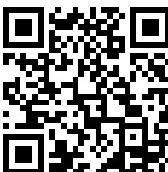


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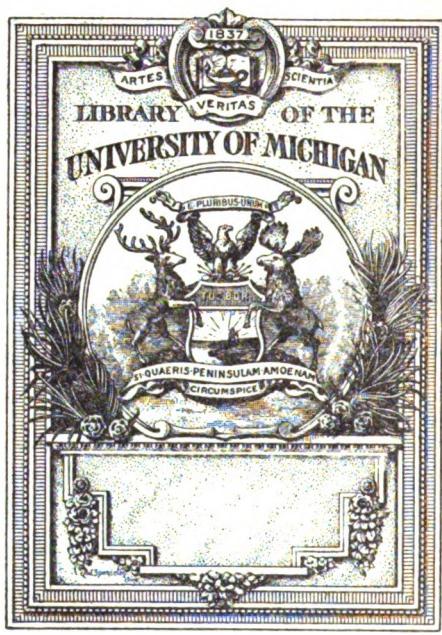
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# TRANSACTIONS

OF THE

## AMERICAN



# PHILOLOGICAL ASSOCIATION.

1873.

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TRANSACTIONS  
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AMERICAN PHILOLOGICAL ASSOCIATION.

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I.—*The Epic Forms of Verbs in áw.*

BY FREDERIC D. ALLEN,  
TUTOR IN HARVARD UNIVERSITY.

I do not bring forward all that I shall say on this subject as new or original with myself. Indeed, very little of it is so. My object is only to give a condensed review of what has been written on the matter by others.\* I desire first to call attention to the necessity of a modification of the prevailing theory of these forms, then to set forth the question of the true solution as it stands among grammarians at present, without pretending to bring the matter, which is not without its difficulties, to an absolute conclusion in all its details.

The verbs in *áw*, the grammars tell us, after being contracted in the well-known manner, undergo often in the Epic dialect a process of *protraction* or *distraction*, in that a contract *w* becomes *ow*, *wo*, or *ww*, and a contract *á* becomes *aa* or *ää*. The two vowels are contracted and then pulled apart again. It is important to observe that this duplication of vowel occurs only in such forms as have the syllable containing the connecting vowel long. Thus, for *ípáet*, *úráovro*, we

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\* Leo Meyer, Kuhn's Zeitschrift, X. 45; Vergl. Gram., I. 292. Dietrich, Kuhn's Zeitschrift, X. 434. G. Curtius, Erläuterungen, p. 96 (2d ed., 98). G. Curtius, Studien, III. 2, p. 399. Brugman, Curtius' Studien, IV. 1, p. 184.

have ὄπᾱͅ, μνώοντο; but for ὄπᾱͅε we do not have any such form as ὄπᾱͅετε, but only the vulgar contracted ὄπᾱͅτε. A single exception will be noted below.

Now such a separation of one vowel into two is very unlikely to happen, being contrary to the laws of phonetic decay, and contrary to experience as well, for nothing of the sort can, I think, be shown to have taken place anywhere else. But to say nothing of this, the theory goes a long way around to explain a very simple phenomenon. Compare ὄπᾱͅεσθαι with ὄπᾱͅασθαι. They differ only in the color of one vowel. Even the accent of the two forms is the same. Why, then, tell us that between these lies the very different ὄπᾱͅσθαι? Why two long steps when one short one will do as well?

Evidently ὄπᾱͅασθαι may be much more simply and naturally accounted for by supposing it to have arisen directly from ὄπᾱͅεσθαι. Instead of being a protracted form of ὄπᾱͅσθαι, it is identical with the uncontracted ὄπᾱͅεσθαι, but with the vowels assimilated preparatory to contraction. The order of development is not ὄπᾱͅεσθαι, ὄπᾱͅσθαι, ἵράασθαι; but ὄπᾱͅεσθαι, ὄπᾱͅασθαι, ὄπᾱͅσθαι. The Epic form is an intermediate one between the original and the vulgar Attic. This is so clear as to convince at first glance. I do not think that any scholar who has given the least attention to modern historical grammar holds to the old view. It arose from the perverse habit of taking the language of Attic prose as the standard, and looking upon all other forms as variations and corruptions of this.

It is, then, almost self-evident that the forms called protracted or distractred are really uncontracted forms with one vowel assimilated to the other, and in this sense Kühner, in his new larger grammar, has treated of them, though without committing himself further as to the explanation of details. But the matter is complicated by the changes in quantity which accompany this assimilation. For if not always, at least almost always, one or both of the contiguous vowels are lengthened. The difficult questions which arise are: How is this lengthening to be accounted for? and in connection with this: How far is the traditional orthography of these forms to be relied on as genuine? And here opinions differ widely.

We shall consider first the lengthening of the first or characteristic vowel, then the lengthening of the second or connecting vowel; lastly, the lengthening of both together.

The cases in which the former or characteristic vowel alone is lengthened are not very numerous, and seem to occur only where the form could not otherwise be brought into the verse; that is, where the syllable preceding the characteristic vowel is long either by nature or position.\* Examples are *ηβάωντες*, *μνώοντο*, *μνώμενος*, *ηβάωιμι*, *δρώοιμι*; *μνάσσαι*, *ηγάσσε*. Neither Leo Meyer nor Dietrich can account for this lengthening in any better way than by saying that it took place from metrical necessity. This, though more excusable at the time they wrote, is unsatisfactory. Metrical convenience was indeed subserved, and had its influence doubtless in determining the form of the words, but it was not the motive power. Curtius explains the lengthening much better as compensative. For it is well known that the contracted verbs have lost a consonant *j* between the stem and the connecting vowel. Most of them are formed from nominal stems by adding the syllable *ja*, to which are then appended the endings. Accordingly *ηβα-jεσθε*, *ηβα-jοντες*, are to be presumed. From these would come *ηβά-εσθε*, *ηβά-οντες*, and thence by assimilation *ηβά-ασθε*, *ηβά-οντες*. It would not be needful to adduce further proof of this view but for a remark which Brugman (*Curt. Stud.*, IV. 1. 182) makes. He seems to think that the length may, in some cases at least, be attributable to the nominal stems whence the verbs come. And it is true that a very large proportion of the verbs in -áw are derived from feminine stems in á; *airiáomai* from *airía*, etc. Brugman thinks it possible that these have often preserved the long á of the stem in the verbal forms. Nor is it a fatal objection to this theory to say that many of the verbs which show this lengthening are not thus derived from stems in long á. For in a partly artificial dialect, like the Epic, it is not at all inconceivable that a type once established should transfer itself to forms where it was not organically justified.

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\* A single exception is *γελώντες*, for which, however, many read *γελοιώντες*.

But there are other and more cogent reasons for preferring the view of Curtius, after all. First, other languages, and particularly the Sanscrit, do not have denominative verbs of the form *ā-jāmi*, or, if the Sanscrit does have a few such, it is merely a phonetic variation, and does not seem to have anything to do with feminines in *ā*. On the contrary, the denominatives, whether formed from feminine stems in *ā* or masculines in *ā*, have alike the form *ā-jāmi* with short *a*. Accordingly, from *ηβā* (*ηβη*) would be formed *ηβā jω* with short *a*. In fact, the distinction between *o* and *a* stems, or rather between short and long *a* stems, is not very firm. Just as the former take the place of the latter in composition (*τυμο-κρατία*) and in derivative nouns (*σκύό-εις*), so also in denominative verbs (*ζημιώ* from *ζημία*). Hence, after all, we shall best refer all the verbs in *āw*, *ēw*, *ōw* to the common fundamental type *ā-jāmi*. Yet another consideration is, that the lengthened characteristic vowel is not confined to the verbs in *āw*, but is found in those in *ēw* and *ōw* as well; as in Lesbian forms like *ἀδικήω*, *ποθῆω* for *ἀδικέω*, *ποθέω*, and on Delphic inscriptions *στεφανώτω*, *ἀπαλλατριώνσα*; etc., to say nothing of traces of the same in the Epic language. Here, of course, no other supposition is possible than that the spirant has lengthened the preceding vowel. Indeed, Georg Curtius in a recent essay (Stud. III. p. 401), after a survey of all dialectic testimony, is led to the conclusion that the disappearance of the spirant *j* left the characteristic vowel in all these verbs long, and that *āw*, *ēw*, *ōw* have everywhere arisen from an older *āw*, *ēw*, *ōw*. This influence of the spirant is therefore in any case a fixed fact, and we shall be safest in applying it to the verbs in *āw* as well as to the others.

We conclude, then, that the lengthening of the first or characteristic vowel is due to the ejection of the semivowel *j*.

The second case we had to consider was the lengthening of the second or connecting vowel. Examples are *airiώνται*, *όρώντες*, *όρώψτε* (*όράοτε*), *airiώρο* (*airiάοι*), *άντιώσται*, *όράςτι*, *όράτι*, *άντιάν*, and perhaps *όράσθαι*, *airiάσθε* and similar forms, though in these last there is nothing to lead us to think the second *a* long by nature, except the analogy of other forms.

The means for accounting for these forms lie already provided, though ignored by Meyer and very imperfectly recognized by Dietrich. The same shifting of quantity in hiatus, which turns *λᾶός* to *λεώς*, *Ἄτρειδᾶο* to *Ἄτρειδεω*, *βασιλῆος* to *βασιλέως*, *πόληος* to *πόλεως*, *Κῶος* to *Κώως*, etc., will transform *όρώντες*, *όράαι* into *όρώντες*, *όράᾳ*. We have only to suppose that the first vowel, lengthened by the influence of the disappearing spirant, afterward transferred its length to the following syllable. Or, if any one prefers to think with Delbrück that in these cases no transfer of quantity takes place, but that the spirant operates directly, now on the preceding, now on the following vowel,\* he can apply that theory to these verbal forms as well. And this explains, too, why the forms with *āa*, *ow*, are so much more common than those with *aa*, *wo*. The forms with shifted quantity are the favorite ones, just as *Κώως*, *Ἄθώως*, *γαλόως* are always used in Homer, never *Κῶος*, etc.; and so always *όρώντες*, never *όρώντες*. Only when the change would give the word such a form as to exclude it from the verse does the poet retain *wo*, *āa*.

While, then, we have ample means for explaining this prolongation of the second vowel, there is yet a suspicious circumstance about the matter. It is this: this lengthening never falls upon a syllable which is not already long by nature or position. It never really alters the quantity of a syllable nor the scanning of a word. Leo Meyer considers this proof that the lengthening is only a fictitious one, that the forms are simply misspelled from a false theory, and in remembrance of the vulgar contracted forms. He boldly claims that we should write *όρώνται* for *όρώντει*, *βούντες* for *βούντει*, *όρόιεν* for *όρόμεν*, and to be consistent he should, though he does not, require *όράει* or *όράαι* for *όράᾳ*.† Now this proposition, startling as it is, has much in its favor. For, one may ask, if there was such a thing as a genuine lengthening of the connecting vowel, why do we not have such forms as *όρώμεν* for *όράμεν*, *αἰτιάραι* for *αἰτιάραι*, etc., which would be extremely convenient for the

\* A view which I cannot accede to, and which certainly does not explain all forms; Curt. Stud., III. 399, Brugman, (l. c.) p. 140.

† Somewhat inconsistently he seems, if I understand him, to admit *ω* before (*v*)*σ*; *δρόωσα*, *δρόωσι*, etc. But if one such form is false, why not all the rest?

poet and fit his verse admirably ? Why must the poet avail himself of this liberty of prolongation only when it can do him no good ? Why, above all, before two consonants, in which situation, as is well known, an organically long vowel loves to shorten itself ?

Curtius replies to Meyer in the "Erläuterungen zu meiner Schulgrammatik," p. 96 (98, 2d ed.), and argues in opposition to the proposed change that ὄρόνται, βοῶντες, ὄρόντει, would not when contracted give the actually existing forms, ὄρῶνται, βοῶντες, ὄρφεν, but rather ὄροῦνται, βοῦντες, ὄροῖεν. Leo Meyer, anticipating this objection, says truly enough that a much older and more general law of contraction is, that two like vowels unite to form the corresponding long. To this Curtius rejoins that the contraction in question does not belong to a very old period of the language, but a comparatively recent one ; that there is no reason for applying to these verbal forms any other rule than to νόος, νοῦς. But just here Curtius does not seem to me to reason with his usual clearness. For we ought not to lose sight of the fact that we have to start, not from the theoretical οο, but from an actual αο. Now, it is matter of solid fact that αο does in hundreds of cases in all dialects contract to ω, and that too where no previous prolongation of either vowel can be thought of. Thus κέρως from κέραος (*κεραο-ος*), θώπλα from τὰ θπλα. And if we follow the current view of the process of contraction we shall suppose that an assimilation prepared the way for contraction : κέροος, θούπλα. In this case even Curtius will be forced to admit that οο, *when it has arisen from αο*, can contract to ω. Or, if he still maintains the impossibility of this, he has still proved nothing but that αο does not pass into ω through the intermediate stage οο. But neither is the intermediate stage necessarily οω, for, in the examples just cited, prolonged forms like κεροως, θωπλα, are, as we have seen, not supposable. Αο must then become ω by some other quite different process, and Leo Meyer needs only to shift his ground a trifle, derive ὄρῶνται from ὄράονται by this process, whatever it be, and still uphold ὄρῶνται in place of ὄρόνται, not considering it as an intermediate form, but rather as a variation of ὄράονται. Indeed, if, setting aside this

older treatise, we recur to Curtius' own latest views, as set forth in the article referred to a while ago, we find that they furnish a perfectly satisfactory explanation of Meyer's forms. For Curtius now holds, as we saw, that the spirant everywhere lengthened the preceding vowel, so that from ὄραγονται we get ὄράονται, ὄρώονται. Now from ὄρώονται we might on the one hand derive the contracted ὄρῶνται, and on the other hand, to be sure, the traditional ὄρώνται, but also equally well by a simple shortening, ὄρώνται, just like Ionic βασιλέος from βασιλῆος. Accordingly ὄρῶνται would come, after all, not from ὄρώονται, but from ὄρώονται, and so the whole objection of Curtius fall to the ground.

While then we cannot find that Curtius has overthrown Meyer's hypothesis by an appeal to the laws of contraction, we nevertheless think there are sufficient reasons for rejecting it. First, that the spirant could and actually did lengthen the connecting vowel in verbal forms, is placed beyond a doubt by Doric forms like ἐμετρίωμες (ἐμετρέομεν), μετριώμενος (μετρεόμενος), Φηλιώμενοι (εἰλεόμενοι); Ahrens, II. p. 208. These are exactly analogous to the ὄρώμεν, etc., which we missed in the Homeric language, as well as to the existing ὄρώνται, etc. To be sure, the existence of these forms in one dialect is not directly conclusive for another; the prolongation might have taken place in Doric and not in Ionic. Yet, as corroborative evidence, they have great weight. But, after all, the consideration which falls most heavily into the scale against Meyer is the overwhelming presumption in favor of tradition, the unlikelihood of a systematic and arbitrary misspelling of forms so frequently recurring. For the forms in question do not occur once, twice, or thrice, but by scores and hundreds. It is true that the earliest copies were written in an alphabet that made no distinction between the long and short o-sound. But the ancients, in transcribing these words into the later alphabet, must have been guided by something more than mere conjecture. For the Alexandrines had manuscripts from all parts of the Grecian world, yet neither from them nor from any other source do we learn of any variation in the spelling of these forms. The ancients, then, read these forms with ω,

and why? From nothing but a well-marked tradition from the lips of rhapsodists and the schools of Epic song. For we must not lose sight of the fact that long after the time of the Peloponnesian war the Homeric poems were not written documents for the eye, but living, spoken words, and that their forms, and doubtless the proper manner of recitation and pronunciation, were matters of fixed oral tradition, and that the delicate ear of the Hellenes would not have tolerated any wholesale alteration in quantities. At any rate, the blunder—if blunder there be—must have lain away back with the rhapsodists themselves. It is plain that under these circumstances nothing but the most cogent necessity should induce us to question the tradition with regard to forms so plentifully attested as these. There is in this case no such need, but rather, as we have seen, Doric analogies, and ample resources for explaining the phenomenon.

As to the question asked above: how it happens that the lengthening of connecting vowel never affects a short syllable, I can answer this only by asking another; how it is that the characteristic vowel never is lengthened before a short syllable. One such case does indeed occur, *μιωόμενος*; but this is exceptional. If we miss ὄρώμεν, ὄράτε, we also miss equally ηβώμεν, ηβάτε. Yet the lengthening of the characteristic vowel is not to be questioned or explained away. Evidently there were some very subtle influences at work which led to the retention of the uncontracted and prolonged forms *only when the syllable containing the connecting vowel was long*, and their rejection in the other case. What these influences were it would hardly be possible to conjecture.

Thirdly, cases occur in which both vowels, the characteristic and the connecting vowel, are lengthened. Examples are: ηβώσσα (*ηβάνσσα*), δρώσσι (*δράνσσι*); μενουρά (μενουράει). These are in truth perplexing forms. Can we suppose that the spirant lengthened both vowels? Brugman (l. c., p. 175) has collected half a dozen words where he thinks digamma to have done this. Some of these are far from absolutely certain, and the phenomenon is at any rate a rarity. Even if proved for *F*, it would not follow that the weaker spirant *j*

could have the same effect. Here, if anywhere, we are inclined to think that the supposition of false spelling would be in place. This supposition would be here far less rash. For these forms are not numerous, and the type last treated of, so greatly preponderating, would furnish an apparent standard to which the others, their true nature lost sight of, might be made to conform; so for *δρώωσι*, *όρώσι* would be a model, for *μενοινάq*, *όράq*. Possibly, therefore, *δρώωνσι*, *ηβώνουσα*, *μενοινδαι* were the genuine forms. Already there is an inconsistency in the traditional spelling, for the optative *δρώοιμι* ought, if treated like *μενοινάq*, to become *δρώψμι*; compare *όρόψμι*.\* Curtius (Stud., III. 400) and Brugman favor this view. The former, indeed, suggests another possibility, namely, a further assimilating influence of the vowels on one another, by which *δρώνουσι* became *δρώωσι*. We can easily understand this in *δρώωνσι*, for the *ov* in this word was not originally a diphthong, only a modified *o*-sound and already long. How it could apply to *μενοινδαι* we hardly see. Finally, I will not undertake to say that it is impossible that the spirant may lengthen both vowels, and so justify the forms perfectly.

The principles adjusted, so far as they can be, several details require mention.

1. Does assimilation without lengthening of either vowel occur? The 1st pers. *όρώ*, partic. *όρώντ*, hardly prove this, since the intention may have been to shift the length to the connecting vowel, though as this was already long it could not be in effect altered. The same is true of the subjunctive *έάqς*, Od. xii. 137. The forms *όράσθαι* and similar ones are, as remarked above, indecisive; but perhaps from the analogy of *όρώντες* we ought to consider the second *a* long by nature.

2. Such forms as *όρώσι*, *όρώσα*, are not to be explained from *όράνουσι*, *όράνουσα*, but directly from the older *όράονσι*, *όράονσα*; thence, with assimilation and lengthening of second vowel, *όρώνσι*, *όρώνσα*; lastly, the *v* was dropped.

3. The infin. *όράειν* makes *όράάv* without *e* subscript. For the *ei* is a very different one from that in *όράει*; it was not originally a diphthongal sound at all (probably from *ee* by

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\* *μενοινάw* is already in order, as the second *w* is long of itself.

contraction), and the ending *ειν* sometimes shortens itself to *εν*. We are taught that the contract infinitives *όραν*, *φλεῖν*, *δηλοῦν*, contain this short ending *εν*. This is not wrong, yet we must not think of this *εν* as the older infinitive ending, but as, after all, a shortened form of *ειν* or *εεν*.\* And we may not derive *όράν* from *όράεν*, because this would violate the rule that lengthening of the latter vowel occurs only in a long syllable.

4. The simple uncontracted form, without either lengthening or assimilation, occurs, though sparingly: *κατεσκίαον* (not otherwise to be got into metre), *πέραον* (imperfect), *ἀοιδάια*, *τηλεθάοντες*, *ναιετάονσι*, *γοάοιεν*, and others, though we can discern no reason why these last should not be *ἀοιδάη*, *τηλεθάοντες*, *ναιετόωσι*, etc. Both *μειδάων* and *μειδάων* occur, the one in the Hymns, the other in the Iliad. For *γοάοιεν* (Od. xxiv. 190) and *γοάοιμεν* (ll. xxiv. 664) Bekker writes *γοώηεν*, *γοώημεν*, following a hint in an Egyptian papyrus.

5. Lengthening of the one or the other vowel may occur without assimilation: thus the former vowel is prolonged in *πεινάων*, *διψάοντα*, *ἀναμαιμάει*: why these should not be written *πεινώων* (like *μαιμώων*), *διψώοντα* (like *ἱβώοντα*), *ἀναμαιμάη* (like *μενοινάη*) no one can say. The latter vowel is lengthened in the singular form *ναιετάωσα*, though this verb elsewhere has nothing but uncontracted, unlenghthened forms. Bekker reads on his own authority *ναιετάονσα*. If the traditional form is correct, it furnishes a good instance of the caprice of the language; compare *ἀοιδάονσα*, *ναιετάωσα*, *ἄντιόωσα*.

6. Yet another variation is seen in *χρεώμενος* (Il. xxiii. 834) for *χρᾶόμενος*, *χρᾶώμενος*; for here the *ᾳ* is weakened to *ε* and forced by synizesis into the following syllable.

7. Peculiar is the form *ἄλώ* (Od. v. 877), imperative of *ἄλαομαι*, for which Meyer has unsuccessfully tried to account. It arose, I conceive, in this way: from *ἄλδεο* came, by a *double regressive assimilation*, *ἄλώο*; this would have contracted into *ἄλών* but for the necessary shifting of quantity, which gave *ἄλώο*, and then, by contraction, *ἄλώ*.

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\* Doric forms which have this ending yet retain the accent of those with *ειν*; *φυλάττεν*.

8. If the Aristarchean form *μενοινήσσαι* (3rd sing. subj.), Il. xv. 82, is right, we must acknowledge an assimilation the reverse of the usual one: we should expect *μενοινάσσαι*; compare *έάρει*, Od. xii. 137. This seems to be on a par with the Doric contraction of *αε* to *η*, some traces of which are seen in Epic, *φοιτήτην* for example.

9. Verbs in *ώ* show some forms identical with those we have been discussing, and to be explained on precisely the same principles. Examples are: *ἰδρώοντες*, *ὑπνώοντες*, *ἀρόσαι*, *δηϊώντο*, *δηϊόφεν*.

10. Similar forms are found in other Epic words of various sorts. *Νηπιάας* (Od. i. 297) is accus. pl. to *νηπιέη* (Il. ix. 491) and stands for *νηπιέας*. Usually it is wrongly explained as distracted from *νηπιάς*. Just so we may read that *φώως* is a resolved form of *φῶς*, but in reality it is uncontracted; *φαῖος* is the original; thence *φᾶος* (shortened to *φῶς*, but plur. *φάεα* retains *ā*), *φῶς*, *φώς*. *Δεδάσθαι* for *δεδάεσθαι*, *φαίντατος* for *φαέντατος* (*φαεινός*), *γοαάσκον*, *ναιετάασκον* for *γοάεσκον*, *ναιετάεσκον*, are cases in point. Instructive is *φάανθεν*, commonly explained as a lengthened form of *φάνθεν* (*ἐφάνθησαν*): it really belongs to *φαείνω*, which is, Buttmann to the contrary, a different verb from *φαίνω*. See Curtius' Etym., p. 278, on the root group *φα*, *φαῖ*, *φαν*. *Φαείνω* stands for *φαῖ-εν-γω*, *φάανθεν* for *φάῖενθεν*. Noteworthy is the verb *κραίνω* with the aorist forms *κρῆνον*, *κρῆναι*, etc. (Od. xix. 567; xx. 115; v. 170) and the exactly corresponding *κραιαίνω*, *κρήνον*, *κρῆναι* (Il. v. 508; i. 41; ix. 101). Add aor. pass. *ἐκράνθην* (Pindar) and *ἐκραάνθην* (Theocr.), verbals *ἄκραντος* (Aesch.) and *ἄκραντος* (Il. ii. 138). With such a correspondence of forms it is not surprising that an "Epic duplication of the vowel" should have been recognized. Yet even here we shall be prepared to find that we have to deal with two verbs. Compare Curtius' Etym., p. 147, where for *κραιαίνω* a nominal stem *κρα-γω* is assumed. *Κραιαίνω* stands for *κραιαν-γω*: the aor. *κραιῆναι* became first *κράῆναι*, then by assimilation *κρῆναι*; the aor. passive *ἐκραιάνθην* became simply *ἐκράάνθην*. *Κραίνω*, on the other hand, has the short stem *κραν-*.

In conclusion I will simply say that I cannot agree with

Curtius and other recent authors of Greek grammars in thinking the true theory of these forms too difficult to find a place in school-books, and preferring to retain for practical purposes the old view. To attempt a complete elucidation of the history of these forms would, indeed, be as ill-advised as in treating of declension or of any other matter, but I can see no reason why we should not tell the truth as far as we go, rather than an error which must afterwards be unlearnt. The main facts might be formulated for use in the class-room somewhat as follows: "The verbs in  $\alpha\omega$ , when uncontracted, commonly show an assimilation of the two concurrent vowels, so as to give for  $\alpha\epsilon$  or  $\alpha\eta$  a double  $\alpha$ -sound, and for  $\alpha\sigma$  or  $\alpha\omega$  a double  $\sigma$ -sound. This assimilation is usually accompanied by a lengthening of one or both of the vowels." In this form I have repeatedly given the explanation to classes and have found no difficulty in making it understood.

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#### ADDENDUM.

The dissertation of Bernhard Mangold "de diectasi Homericā, imprimis verborum in  $\alpha\omega$ ," printed in Curtius' Studien, VI. 1, reaches me just as the foregoing pages go to the printer. The author has discussed these forms at greater length and much more in detail than I have attempted to do. As his paper and mine have arisen quite independently of one another, it may be worth while to note the chief points of agreement and disagreement.

He coincides with me in his view of assimilation of the vowels; also in explaining the lengthening of the first vowel as due to the spirant in all cases. With respect to the lengthening of the second vowel, he thinks with me, and for similar reasons, that Curtius' argument against Meyer is invalid: he also holds with me that Meyer's presentation of the subject was unsatisfactory and inconclusive. But here we separate. For he rejects Curtius' theory of an interchange of quantity between the first and second vowels, and denies any effect of

the spirant whatever on the second vowel. He urges in support of this: 1st, that the examples of transfer of quantity all show a change of  $\bar{a}\omega$ ,  $\eta\omega$ , into  $\epsilon\omega$ , or  $\eta\alpha$  into  $\epsilon\bar{a}$ , whereas no certain example of the passing of  $\bar{a}\bar{a}$ ,  $w\omega$ , into  $\bar{a}\bar{a}$ ,  $ow$ , or of interchange between two vowels *alike in color*, can be found; 2nd, that the Homeric forms show that the stem-vowel  $a$  was *shortened* before assimilation took place; 3rd, that only those forms are found assimilated in which the second vowel was already long by nature or position. With regard to the first objection, I believe that  $Ków\zeta = \kappa\bar{a}\omega\zeta$  and others furnish just the examples that M. misses, though he has another way of disposing of them. At any rate, he shows no reason why the interchange might not take place between like vowels. Moreover, we might suppose that the interchange of quantity preceded the assimilation, so that it would not be between like vowels, after all. M. himself allows this in case of  $\phi\omega\zeta$ , which he derives thus:  $\phi\bar{a}\zeta$ ,  $\phi\bar{a}\omega\zeta$ ,  $\phi\omega\zeta$ ; and just the same method is applicable to the verbal forms. Even M. does not lay much weight on this objection, and says that it alone would not prevent him from adopting Curtius' theory. To pass to his second objection, he is here much in error. He finds that of those verbs in  $\acute{a}w$  which do not admit assimilation all but three ( $\acute{a}v\alpha\mu\acute{a}\bar{w}$ ,  $\pi\epsilon\iota\bar{r}\acute{a}\bar{w}$ ,  $\delta\psi\acute{a}\bar{w}$ ) have shortened the  $a$ , and hence he concludes that verbs which do admit assimilation must have shortened the  $a$  first. But this by no means follows, and  $\iota\beta\acute{w}\omega\eta\tau\alpha$ , etc., prove that assimilation did sometimes take place before the stem-vowel was shortened. He goes on: "nam id profecto animum inducere non possumus, linguam, postquam ex fastidio quodam vocalium concurrentium eas inter se assimularit, juxta has formas etiam integras servasse earumque  $\bar{a}$  longam posteriore tempore corripuisse." But the language did do just this, witness  $\iota\beta\acute{w}\omega\eta\tau\alpha$  and  $\pi\epsilon\iota\bar{r}\acute{a}\eta\tau\alpha$ .\* However, we may, as suggested above, explain  $\dot{\omega}\rho\acute{w}\eta\tau\alpha$  without having recourse to  $\dot{\omega}\rho\acute{w}\eta\tau\alpha$ ; namely, from  $\dot{\omega}\rho\acute{a}\eta\tau\alpha$ . M. observes this, but says: "sed tum quomodo formae  $\iota\beta\acute{w}\omega\eta\tau\alpha$ ,  $\mu\nu\acute{w}\omega\eta\tau\alpha$  ortae sint, omnino non intellegitur." On the contrary, it is his theory which

\* There may have been some special reason for the non-assimilation of  $a$  in  $\pi\epsilon\iota\bar{r}\acute{a}\bar{w}$ ,  $\delta\psi\acute{a}\bar{w}$ : these verbs were peculiar in their Attic contraction.

renders these forms inexplicable. They may be readily derived from *ἱβάοντα*, etc.; the quantity could not interchange, simply because it would exclude the word from the verse. There is not the smallest difficulty in accounting for all forms on the theory of transfer of quantity. In fact, we may have our choice between two orders of development; either:

*Original* { *πεινάοντα* } *reject* { *πεινάοντα . . . cannot shorten a,*    *πεινάοντα.*  
*ναιεράοντα* } *assim.* { *ναιεράοντα . . . shortens a,*                      *ναιεράοντα.*  
*ἱβάοντα*        } *admit* { *ἱβώντα . . . . cannot transf. qu.,*    *ἱβώντα.*  
*օράοντα*        } *assim.* { *օρώντα . . . . transfers quantity,*    *օρώντα.*

or:

*Original* { *πεινάοντα* } *cannot shorten* { *πεινάοντα . . . rejects assim. πεινάοντα.*  
*ἱβάοντα* } *nor transfer,* { *ἱβάοντα . . . admits*    "    *ἱβώντα.*  
*ναιεράοντα* *shortens,*                      *ναιεράοντα . . . rejects*    "    *ναιεράοντα.*  
*օράοντα*        *transfers,*                      *օρώντα . . . admits*    "    *օρώντα.*

Although in the preceding pages I have with Curtius assumed the former of these theories, I am now inclined, for reasons I will explain below, to adopt the latter. The third objection we have already sufficiently answered, page 12.

Having thus, for very insufficient reasons, as I conceive, rejected the notion of transferred length, Mangold explains the quantity of the second vowel differently in different forms.

1. In the infin. (*օράων*) he denies altogether the length of the second *a*, on ground that it stands everywhere before two consonants or in a principal caesura.

2. With respect to the forms with *οωντ-* and *οψ-* he takes, after all, substantially Meyer's ground, claiming that they stand by a blunder for *οοντ-* and *οοι-*; he recognizes, however, as we have done, that this cannot be a mere clerical error of the transcribers, but thinks it due to a vicious pronunciation of the rhapsodists themselves.

3. As to the 2d and 3d pers., like *օράς*, *օράῃ*, he entertains the singular view that the organic lengthening of the second vowel, which belongs properly to 1st pers. only (Sanscr. *-ajāmi*, *-ajāsi*, *-ajāti*), was extended in Greek to the 2d and 3rd pers. (as, indeed, was the case in *τιθημι*, *ιστημι*), and that we have to start from *օρα-η-σι*, *օρα-η-τι*, instead of *օρα-γε-σι*, *օρα-γε-τι*, as commonly assumed. Of course he must suppose that this *η* when once introduced did not maintain itself, but made great haste to shorten itself again to give us *φιλέεις*, *στιχάεις*, etc.

The forced nature of this explanation does not escape its author, who admits that he proposes it with hesitation.

4. The forms in *ωσι*, *ωσα*, M. follows Dietrich in deriving from *ανσι*, *ανσα*, by a double assimilation, first progressive, then regressive; *όρανσι*, *όράσι* (since *ω* is nearer to *α* than *ον* is), *όρώσι*. But the first of these assimilations I cannot find probable, and submit that my series *όρδονσι*, *όρώνσι*, *όρώσι*, sustained by the analogy of subj. mood, is simpler. M. says (p. 152, note) that *vauerávōσα* proves that *ονσι* became *ονσι* before *α* was assimilated. That may be, though *vaueráw*, being one of those verbs which for some unknown reason admits neither assimilation or lengthening, can prove nothing about those which do. Yet there is no difficulty in supposing that transfer of quantity took place, where it did at all, before *νου* dropped its *ν*. Only it will be perhaps as well, since the assimilation (preparatory to contraction) would belong naturally to a later period, to assume that transfer preceded assimilation in point of time, adopting the second of the tables exhibited above. On this plan we might suppose:

*vαιετάονσι* *shortens*, *vαιετάονσι* }    *v. dissapp.* { *vαιετάονσι*.  
*όράονσι*      *transfers*, *όράωνσι* }      *όράωσι*, *assim.* *όρόωσι*.

On the whole, therefore, I cannot say that my views have been much modified by the perusal of Mangold's paper. Yet it is an able one, showing great diligence, and, except in the points spoken of, eminently satisfactory.

## II.—*Studies in Cymric Philology.*

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In preparing the series of philological notes of which this paper is a continuation, it is not my plan to arrange them methodically, or according to connection of subject, but rather to discuss each question as it occurs, or whenever sufficient data for its discussion have been found.

### XXI.

In the earliest examples of Welsh writing, there is a remarkable fluctuation, in many words, between *o*, *e*, and *i*. This I venture to explain by saying that in the early unsettled orthography each of these letters, besides its usual sound as in Latin, was made to represent a sound for which the Latin alphabet had no distinctive character; I mean the neutral vowel either pure or in some of its modifications, in other words, something of the class known as obscure vowels. In support of this view I observe that from some time in the thirteenth century on we find *y* regularly replacing *o*, *e*, and *i* in these cases of fluctuation, and that it is in precisely these cases that *y* has its obscure sound in modern Welsh.

In the Oxford and Cambridge glosses *i* occurs most frequently in the places now occupied by the obscure *y*, though there are many examples of *e* and *o*. In the Luxemburg glosses *o* is generally found in such places. In the Venedotian Laws *e* decidedly predominates. In the Black Book of Caermarthen *i* predominates in some pieces and *y* in others.

Examples: *bodin* in the glosses, *bedin* in the Laws (104), *bitin* in the Black Book (55), now *byddin*, army; *do-* and *di-* in the glosses, *de-*, rarely *do-*, in the Laws (2,124), *di-* and *dy-* in the Black Book (10), now *dy-*, synonymous with Latin *ad*; *con-* and *cen-* in the glosses, *ken* in the Laws (36), *cin-* and *cyn-* in the Black Book (4), now *cyn-*, equivalent to Latin *con*; *Ougen* and *Eugein* in *Chronicum Cambriae* (X and 9),

*Owein* in the Black Book (49), later *Ywain*, Owen; *Broceniauc* and *Bricheniauc* in Chron. Camb. (13, 16), *Brecheniauc* in Annales Cambriae (32), now *Brycheiniog*, Brecknock; *Cinan* in Chron. Camb. (12), *Kenan* and *Conanus* in Ann. Camb. (12, 32), later *Cynan*, a personal name; *Rodarcus* in Vita Merlini, *Retherc* in the Laws (104), *Ryderch* and *Ritech* (leg. *Riterch*) in the Black Book (19, 21), modern *Rhydderch*; etc. This fluctuation between *o*, *e*, and *i* (rarely *a* or *u*) can be illustrated at indefinite length, being in fact co-extensive with the prevalence of the obscure *y* in later orthography.

In modern Welsh *y* has two sounds. In final syllables, in most monosyllables, and in the diphthong *wy*, it has a slender sound like that of English *i* in *him*, not quite so slender as the Welsh *i* is sometimes heard. In other situations, with few exceptions, it has an obscure sound. This, as heard in most parts of Wales, is simply the neutral vowel; but in some districts it does not differ widely from the slender *y*, and yet may be said to approximate to the neutral vowel. Some have discarded the obscure sound of *y* and held that it is of very recent origin; but this is an egregious error.

The distinguished Edward Lluyd carefully dotted the *y* in all those cases where it now has the obscure sound; and that it was the neutral vowel two centuries ago appears from his statement that *y* when dotted was to be pronounced "as the English *i* in the words *third, bird*; *o* in *honey, money*; *u* in *mud, must*" (Arch. Brit. 2).

In middle Welsh *y* had two sounds as now. One was a slender sound, for as such it attenuated a preceding *a*; thus *gelyn*, enemy, from *gal, gwledyd*, i. e. *gwledydd*, countries, from *gwlad*, etc. The other was an obscure sound, which obtained even in final syllables in cases where it is now suppressed in orthography, thus *gwaladyr*, ruler, modern *gwaladr, trwyadyl*, sprightly, modern *trwyadl* (Herg. 230). These words, and others of like endings, are derivatives; hence if *y* had been slender here it would, by a law of umlaut in Welsh, have attenuated the preceding *a*. It must be the neutral vowel, or something closely approximating to it, that *y* represents in

such middle Welsh examples as *aryf* for *arf*, arm, *dyragon* for *dragon*, dragon (Myv. I. 161), and *baryflwyd* for *barflwyd*, gray-bearded (Herg. 244). In verse *aryf* is a monosyllable, *baryflwyd* a dissyllable, etc.; the *y* in such cases being simply inserted to mark the quasi syllabification arising from the imperfect joining of two consonants, as if in English we should sometimes find *chasum* written for *chasm*. In such cases the neutral vowel, very short, is what we naturally hear. Again, in Codex B of Brut Gr. ab Arthur, which bears marks of the Demetian dialect, we find such spellings as *gyireu* for *geireu* (Myv. II. 258), *dryigeu* for *dreigeu* (262), *kyissaw* for *keissaw* (271), *anyirif* for *aneirif* (334), etc. This singular diphthong, *yi*, is explained by the fact that in some parts of South Wales, at least, the *ei* in these words is still pronounced as if *e* represented the neutral vowel.

Add these indications to those before seen in the earlier orthography, and I think a high antiquity will be considered as fairly established for the neutral vowel in Welsh. In the oldest copy of the Laws the secondary office of representing it, as before stated, was assigned to *e*; but the slender *y* was already in use. This distinction of *y* and *e* coincided every where so exactly with the modern distinction between the two sounds of *y* as to afford one of the most striking illustrations of the slowness with which the Welsh language has changed for the last seven hundred years. Thus *tredyd* (60), third; *hyd* (286), hart, plural *hedhod* (38); *e dyn* (50), the man, plural *denyon* (18); *en llys* (10), in the palace; etc.

## XXII.

In the glosses we find *mogou*, i. e. *mongou*, modern *myngau*, plural of *mwng*, mane; also *lichou* (incorrectly printed *laichou* in the first edition of Zeuss), modern *llychau*, plural of *llwch*, lake, (*luch*, in Stevenson's Nennius, referred to the tenth century); also *creman*, modern *cryman*, reaping-hook, from *crwm*, bent. Here we see the obscure *o*, *i*, and *e* replaced by the later *y* obscure; and it becomes apparent that in old Welsh, as now, the umlaut of *u* (*w*) was an obscure vowel, at least in cases where the first vowel of the added syllable was not slender.

## XXIII.

The ingenious author of the Literature of the Kymry has unaccountably fallen into the error (453) of supposing that *dd*, as a sign for the infected *d* sound, was not in use before it was adopted by Dr. Davies, or until after 1620. By this error, which amounts to more than 200 years, he has widely misled himself and others in judging of the antiquity of certain MSS. As authority for his statement he refers to Lluyd; but in justice to Lluyd it should be noted that what he does say (Arch. Brit. 227) is that “*dd* was introduced to express this sound about the year 1400.” In fact it had begun to be used somewhat earlier; for it appears in the Record of Carnarvon, which is authoritatively referred to the fourteenth century (Z. 139).

## XXIV.

In Codex A of the Laws *dh* is not infrequently used for *th*, and sometimes also, as if by a confusion of the two sounds, for what is now *dd*. But as a distinctive character for the latter sound *dh* does not appear to have been used till modern times. William Salesbury in 1567 expressed a regret that it had not been adopted in preference to *dd*. Lluyd tells us that “in the reign of Queen Elizabeth Dr. J. D. Rhys, Dr. D. Powel and others used *dh*, which was afterwards rejected by Dr. Davies and *dd* restored.”

The supposed examples of this use of *dh* cited by Zeuss from the printed edition of the Mabinogion (with a query as to whether they are to be found in the MSS.) are all deceptive. They are *nodho*, *rodho*, *rodhom*, *rydhau*, *rydhaa*, *rydhaf*, *rydhær*. In every one of these examples the *h* was intended by the scribe to be pronounced separately from the *d*. The first three belong to the present subjunctive, which, in middle Welsh, very commonly inserts *h* before the terminations (Z. 512); thus *nodho*, i. e. *nodd-ho*, modern *noddo*. The remaining four are parts of the same derivative verb in *-äu*, and all verbs of this class often insert *h* before the final *a* of the stem.

## XXV.

In the earliest Welsh MSS. *u* (or *v*) represents two vowel sounds. One was the sound of the modern English *oo*. To distinguish this a *v*, modified so as to resemble the figure 6, was introduced in the latter half of the thirteenth century, and this afterwards gave place to *w*. To express the other sound, *u* was retained. It was probably the sound of the modern French *u*. It came generally from primitive *o* or *ū*; thus *dydd sul*, dies solis, *dydd llun*, dies lunae. In modern Welsh it does not differ from the slender *y*; but it would be contrary to the evidence to assume, as some have done, that the same thing was true in middle Welsh. For example, *punt* and *hynt* now rhyme perfectly; but the mediæval poets carefully kept *y* and *u* separate in their rhymes. Moreover *y*, as representing a slender sound closely approaching *i*, regularly attenuated a preceding radical *a*, but *u* did not produce this effect; thus *iachus*, healthful, *iechyd*, health, both from *iach*, healthy.

## XXVI.

Dr. Owen Pughe says we sometimes find *-i* in early writers as a termination of the third person singular, present (or future) indicative active. I have not found it. But of *-i* for the usual *-ei* (modern *-ai*) of the imperfect, I have found evident examples. Thus in the Gododin (B. An. 68), *Ni nodi nac ysgeth nac yegwyt*, nor spear nor shield availed; in Gwalch mai (Myv. I. 198), *Amser ym ceri ef carwn Dafydd*, the while he loved me I loved David; in Gwynfardd Brycheiniog, a poet of the twelfth and thirteenth centuries (ib. 272),

Wynepclawr, ditawr, dim ni weli,  
Pefychwys, tremwys, drwy uot Dewi.

Blank-faced, dispirited, he nothing saw,  
He brightened, he had his sight, by the will of Dewi.

This *-i* (which, by the way, is not given in Zeuss) naturally associates itself with the plural terminations, *-im*, *-ich*, *-int*, of this tense, often seen in the early poets instead of *-em*, *-ech*, *ent*.

## XXVII.

In the second edition of Zeuss an attempt is made to construe *nodi*, in the line just quoted from the Gododin, as infinitive. The passage is further complicated by connecting it in construction with the next two lines, which really form an independent sentence. They are as follows:

Ny ellir anet ry vaethpwyt  
Rac ergyt catvannan catwyt.

In *vaethpwyt* we have an example of the provection of the mediæ after strong consonants, which I pointed out in Art. XI., *pwyt* being for *bwyd*, food. *Catwyt* is not the perfect passive, as it has been rendered, but another form of the infinitive, of *catw* (modern *cadw*, to keep, to defend) after the analogy of *dywedyd*, *dychwelyd*, etc. I find many instances where *catwyt*, or *cadwyd*, is unquestionably used as infinitive; take the following from Einion Wan (Myv. I. 335), a poet of the first half of the thirteenth century: *roi e wann yw e annwyt, a rac pob cadarn catwyt*, it is his nature to give to the weak, and to defend him against every one that is strong. This being premised, the construction of the above somewhat vexed passage becomes perfectly simple and idiomatic: *ni ellir cadw annedd rhy vaethwyyd rhag ergyd catvannan*; it is not possible to defend a too festive house from the blow of *catvannan*.

'I have left *catvannan* here untranslated, because its meaning is hardly settled. The word occurs three times in the Gododin, but is found nowhere else. In some of the MSS. it is written, in each case, *catvannau* or *cadfannau*, as if it were the plural of *cadfan*; but this word also is unknown elsewhere, except as a personal name. It is usually defined as "warrior," while *catvannan* has been rendered "warlike tumult." But without the initial inflection should we not have *cat Mannan* and *cat Manaw*? Now *Manann* was the Gaelic and *Manaw* the Welsh name for a district at or near which the battle of Catraeth here described was fought, as well shown by Skene. It is the *Manau Guotodin* (*Manaw Gododin*) of Nennius. I think, therefore, we should translate thus: 'it is not possible to defend a too festive house from the blow of the host of *Manann* (or *Manaw*).'

I know that *cad* usually means battle ; but, like the Irish *cath*, it also sometimes means, in the earliest Welsh, a host or array ; thus in the Gododin : *gwyr a aeth Gatraeth yg cat yg gawr*, men went to Catraeth in array and with shouts.

### XXVIII.

It has been assumed that in the Latin *nona hora*, ninth hour (the designation of the hour ending near the middle of the afternoon), we have the originals of the two Welsh words *avr*, hour, and *nawn*, afternoon. But this would be contrary to historical laws ; for primitive *ō* passed into Welsh *u*, and on the other hand Welsh *aw* came generally from primitive *ā*, sometimes from *av*. The Welsh form *avr* (*aur* in an old Welsh gloss) and the Cornish form *er* together point distinctly to *ār* as the ancient British form of this word.

As to *nawn*, it suggests the Sanskrit *navan*, nine. Dr. Aufrecht is said to have inferred that Welsh *naw*, nine, like its Sanskrit equivalent, must have ended in *n* from the fact that it often nasalizes the initial of the word following it. Do we not see this earlier Welsh form still preserved in *nawn*? This will explain the anomalous Armoric *naontek*, nineteen. The Welsh *prydnaawn*, afternoon-time, would thus mean, primarily, ‘the time, or hour, of nine.’ No doubt the Britons borrowed this mode of reckoning the hours from the Romans ; but in doing so they would naturally use their own numerals.

We have an analogous case in *dawn*, gift, which is not from the Latin *donum*, for this should have given us *dun*, but indicates an original *dān*, with which we are to compare Irish *dān*, gift, and Sanskrit *dān*, gift.

### XXIX.

The Welsh have an historical tradition that the original British name of Pelagius, that by which his adopted Latin name was suggested, was a word signifying “sea-born,” and that in fact the name was Morgan. In this precise form the legend involves an inconsistency, which I wonder the acute Price, in writing his History of Wales, did not see and point out. The old Welsh form of *Morgan* was *Morcant* (Chron.

Camb. 8), which could not mean “sea-born.” But if we search among the known old Welsh names for one which admits of this meaning, we shall find it in *Morgen*. Now *Morgen*, in the transition to middle Welsh, not later than the eleventh century, would become disguised as *Morien*. If, then, we can find, in early writers, a Morien commemorated whose history may be shown to conform, in distinctive points, to that of Pelagius, we shall have a remarkable proof of the antiquity of the tradition; for it must ascend to the period when *Morien* was *Morgen*, and suggested the meaning *sea-born*. Such proof is not wanting.

In a chronicle attributed to Caradoc of Llancarvan, published among the Iolo MSS., we are informed, under the date 380, that “about this period, Morien, the son of Argad the bard, flourished,” that “the delusion of Morien (*hud Morien*) constituted one of the three ruinous delusions of the Island of Britain,” and that through it “baptism and sacrifice ceased in Britain, where the whole population became unbaptized Jews.” The reader of ecclesiastical history will see some exaggeration here, but he will hardly question that the “delusion of Morien” was the Pelagian heresy, especially after reading, a little further on, the following conclusive statement: “In 425 St. Germanus came from Gaul, with St. Lupus, to Britain, to renew baptism, sacrifice, and a right belief in Christianity, which had fallen into decay.” It will be remembered that Germanus and Lupus were sent to Britain, by the bishops of Gaul, for the express purpose of resisting the Pelagian heresy, which had grown up in this interval, from 380 to 425.

### XXX.

The nasal infection of *t* after *n*, as in *hanner* for *hanter*, half, including also the simple disappearance of final *t* after *n*, as in *gan* for *cant*, with, took place chiefly in the transition from old to middle Welsh. In some points this change went on further, but in others it was arrested in the twelfth century, and notably in the verb-endings *-int* (or *-ynt*) and *-ant*. In the unquestioned productions of the twelfth and later centuries we very seldom find *-in* for *-int* or *-an* for *-ant*; but in a considerable

portion of the literature for which a higher antiquity is claimed these contractions are quite common. It is so in the Gododin; and at first view this would seem to show that its composition, or that of some portions of it, could not be referred with much probability to a more remote period than the eleventh century. But on examining the examples in their connections I find evidence leading to the opposite conclusion.

I find that in all the cases where verbs with these contracted terminations occur at the end of lines, seventeen cases in all, they are made to rhyme with one another or with other parts of speech in which final *t* after *n* has likewise disappeared. I find that *-an* and *-in* (or *-yn*), where they are neither verb-endings nor contractions, occur at the end of lines over one hundred and twenty times. Now, where the number is so large, why should not an occasional verb in *-an'* or *-in'* be found rhyming with them? The natural conclusion is that the poem was composed when *-in* for *-int* and *-an* for *-ant* were yet uncommon if not unknown, that where these contractions occur in it they are due to the hands of scribes who copied after this kind of nasal infection had become popular, that is, in the eleventh century.

It is necessary to examine two particular examples which may at first sight look doubtful.

One stanza of the Gododin, numbered LXXXII. by the translator in Skene, begins thus:—

Ef gwyrhodes tres tra gwyar llynn,  
Ef lladei val deur dull ny techyn.  
He repelled attack over a pool of blood,  
He smote like a hero such as yielded not.

Here a verb in *-in'* rhymes with the substantive *llynn*. Now if among so very large a number of examples in point we should find one real exception, it would necessarily show nothing more than what we knew before, namely, that in old Welsh there were already certain beginnings of the nasal infection. Really, however, there is no exception. The earlier form of *llynn*, pool, liquid, (though it is *linn* in Nennius) must have been *lint*; compare Irish *lind* (Stokes' Irish Glosses, p. 58). This conforms to the analogy by which Welsh *plant*, children, is Irish *cland*, tribe.

Two of the stanzas of the Gododin, numbered LXXXVIII. and LXXXIX., are so much alike in every line except one that they must be considered as two versions of the same original. The text of the former is in several places corrupt, utterly so in the third line; and I therefore give the other:—

Gueleys y dull o bentir a doyn,  
 Aberthach coelkerth a emdygyn;  
 Gueleys y deu oo eu tre (re) ry gwydyn  
 O eir nwython ry godessyn;  
 Gueleys y wyr tylliauwr gan waur a doyn,  
 A phenn dyuynwal vrych, brein ae knoyn.

In all the translations I have seen, the *a doyn* at the end of the first line (rhyming with verbs in *-yn*) is considered a local name, Adoyn. But I think there can be no reasonable doubt that it is simply a relative clause for *a doynt*, ‘that came.’ I translate as follows:—

I saw the array that came from Cantyre,  
 It was as victims for the sacrifice they brought themselves;  
 I saw the two who fell apart from their tribe,  
 Who by the command of Necton had offended;  
 I saw men with great wounds who had come with the morn,  
 And the head of Domhnal Brec—the ravens were biting it.

From the third line I cast out *re*, which seems to be repeated, in later spelling, in the verbal particle *ry* (here, as often, used with a relative force), and, indeed, *re* does not appear in most of the MSS. In respect to the use of *tre* (i. e. *tref*, old Welsh *treb*) in the sense of tribe, see, in the Book of Taliesin (206), the example *deudec tref yr Israel*, the twelve tribes of Israel, also compare Irish *treabh*, tribe.

In the fifth line, *y*, after *gueleys*, is evidently the pronoun *i.*

Mr. Stokes accepts Price’s identification of Dyvnwal Vrych with Domhnal Brec, or, as the name was written later, Donald Brec. I therefore wonder that, with his quick eye for Northern localities, he does not discover Cantyre (*cenn tire*), of which peninsula Domhnal Brec was king, in the equivalent Welsh name *Pentir*, ‘head of land,’ seen, with initial inflection, in the above stanza. Instead of that he proceeds to locate “the height of Adoyn,” which he finds in a Dun or Down!

### III.—*On Koch's Treatment of the Celtic Element in English.*

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It is my purpose to make some remarks on Koch's treatment of the Celtic element in the English language. C. Friedrich Koch is the author of a *Historische Grammatik der Englischen Sprache*, in three volumes: the first treating of inflection, the second of syntax, and the third of derivation. The "English language," as he understands it, includes all periods, from the Anglo-Saxon down to the present. Under each head of inflection or syntax he gives, first, the Anglo-Saxon form or usage, then the Semi-Saxon—or, as he calls it, the nearer Anglo-Saxon (Layamon's Brut and the Ormulum)—then the Old English (Robert of Gloucester and Robert of Brunne), then the Middle English (Chaucer, Piers Ploughman, Wycliffe), and last, the modern English, from Shakespeare to Dickens. The third volume is in two parts, the first treating of the Teutonic part of the language, the second of its foreign elements. In the latter part the author begins with Celtic (13 pages); then follows Latin (19 pages), then Norman-French (168 pages, the bulk of the book), then other foreign words (6 pages), and finally, proper names (8 pages).

As regards the Celtic element, his opening statements of a general character are tolerably correct. But he says that of the old Celtic languages only the Old Irish is preserved in monuments, which are referred to the 8th or 9th century; while of the rest there are found only single words, in Latin, Romance, and other languages. He is apparently not acquainted with the Old Welsh glosses (at Oxford and Luxembourg) of about the same period as the Old Irish monuments. From the 12th century, the Welsh monuments are sufficiently copious. The Cornish and the Armorican, or Breton, are little, if at all, later. His own studies in Celtic are apparently confined to recent languages. He makes no attempt to show that the Celtic words given in his etymologies are old, in

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\* Written out from the author's rough and abbreviated notes (of March, 1872).

substance or form. His failure to do so is not unimportant, if (as I suppose) many of these words are borrowed from English, and borrowed in recent times. He declares that a not inconsiderable part of the Celtic language has passed into English. The question is, what "a not inconsiderable part" may mean. One may claim that two dozen words are a not inconsiderable part of a language. With my ideas, I should say, rather, that the number of words which the English can be proved to have taken from Celtic is inconsiderable; but that a not inconsiderable part of those which Koch gives as Celtic are such that their Celtic origin cannot be proved, or made probable. Still, it may be said in favor of his Celtic etymologies, that they are not generally very bad. He is much more moderate and sober than many etymologists, in this field of philological study. He does not derive from Celtic many words which clearly came from other sources. Still, it cannot be denied that his procedure is uncritical, and that a good many things that he asserts are decidedly objectionable.

I will preface with a few general remarks of my own on English borrowing from Celtic.

Celtic words may have come into English, 1. directly, from intercourse with Celtic men, or speakers of Celtic; or 2. indirectly, through the French. And directly, in two ways: either from intercourse with Celtic persons remaining and living in the midst of the Anglo-Saxons at the time of the Conquest; or from intercourse with unconquered Celtic tribes on the western frontiers. Now as regards the first medium, or Celtic persons remaining among and living in the midst of Anglo-Saxons after the Conquest: if there were any such who retained their language for a succession of generations, this language would probably after a time begin to affect that of the conquerors. Some words—not many, perhaps, but doubtless some—would pass into the use of the Saxons, and appear as Anglo-Saxon words. If there are words found in Anglo-Saxon monuments which do not occur in other Teutonic languages, but do occur in Celtic (or rather, in Welsh and Cornish), then there is reason to suspect them of such origin. Words of this class are certainly very few. *Glen* may perhaps

be one. It is said to be found in Anglo-Saxon (not in the poetry; Grein's vocabulary does not contain it), and to be unknown to other Teutonic idioms. It does exist in Welsh (*glyn*); but whether in Old Welsh, is a question. It wears the aspect of a Celtic word, and might naturally enough be taken from the speech of a conquered people: taken first, it may be conjectured, in connection with some particular valley or valleys (Glen this, Glen such a one), and so at length received as a general name for all glens.

Next as regards the second medium, or independent Celtic tribes, living as neighbors to Saxons and holding more or less intercourse with them. Such tribes, we know, there have been, and such intercourse, ever since the Anglo-Saxon conquest. Though the best and largest part of the island was conquered within a century and a half, yet native tribes existed unsubdued along the whole western coast, from Cornwall to the Highlands of Scotland. For a long time, however, the intercourse was very slight, and, being chiefly hostile, was little calculated to carry words from one people to the other. During the last few centuries it has been greater: certainly many English words have passed into the Celtic of Wales (Welsh), into Irish, and into the Gaelic of the Scotch Highlands. It is certain also that some words of these languages have passed into English. *Bard, brogue, clan, druid, plaid, shamrock, shanty, whiskey*, are undoubted examples. The words most likely to be thus taken, of course, are those denoting Celtic objects, Celtic persons, things, and activities. Such is the case with the words just mentioned; they all have this character. In regard to other words—those which have no special connection with Celtic persons or matters—we may say that the transfer of such from the Celtic languages to English is far less probable. There is very little learning of Celtic by Englishmen, and very little disposition on their part to imitate Celtic modes of expression; there is no Celtic literature known and read by them, and hence no action of Celtic tongues on the body of English speakers. Koch thinks that the neighboring dialects—the dialects of Englishmen or Scotchmen living next to Celts—might absorb many Celtic

words, which would spread from them to other English dialects in their vicinity, and from these to yet others, and so at length would make their way into the general language. This is very conceivable, certainly, but not yet proved. Koch has not tried to prove it. But it is a point which deserves to be investigated. In advance of such investigation, we are not authorized to take it for granted. It would be easy for English scholars to examine the dialects of Devonshire, Gloucestershire, and Cumberland, in order to see whether they have any special relation to Celtic. Such a work would be more useful than most of what they have done for Celtic philology. I ought, perhaps, to remark that words which as now used have no perceptible reference to Celtic conditions may once have had such; thus, *dirk* and *gown* may originally have denoted a particular weapon or article of dress used or worn by some Celtic people. This should of course be considered in estimating the probability of Celtic etymologies.

Again, Celtic words may have come to us indirectly, through the French. The Latin introduced into Gaul among Celtic people might receive Celtic words in the two ways just pointed out and distinguished. There is every reason to think that the Gauls (the great body of them) soon learned Latin of their conquerors. But some of their own native words may have been retained in the dialectic popular language of the provinces, which, affected afterwards by the Teutonic conquest, appears at last as Old French. Thus, we find the word *basket* in Old French; but Juvenal and Martial speak of *bascauda* as British, “*Britannica*”; the Welsh, in fact, still has *basgawd*. Very possibly the word may have been Gallic as well as British, and thus have passed through the provincial Latin of Gaul into Old French. But Celtic words might come into French another way—namely, by intercourse with a neighboring tribe which continued to speak Celtic, and speaks it still (mixed with many French words) at this day. The tribe referred to are the Bretons, inhabitants of Bas-Bretagne (Brittany) in northwestern France. The language of this people is probably not the descendant of ancient Gaulish; it is too much like Welsh for that. It is supposed to be derived from Welsh

immigrants who came over from England during the Anglo-Saxon conquest in the 5th or 6th century, and settled on the coast of Gaul, in what was then called Armorica ('sea-coast,' from *ar*, 'upon,' *mor*, 'sea'). It is not unlikely that their language (Armorican) may have furnished some words (though probably not many) to the French, before French was brought over to England by the Normans in the 11th century. If there are French words which do not occur in other Romance languages, but do occur in Armorican, it is natural to explain them as thus derived from Armorican, provided they are words such as a people like the French might naturally take from a people like the Bretons. The whole number of Celtic words taken into French in either of these two ways, from the old Gauls or from the Bretons, is certainly not large. In regard to words common to French and other Romance languages, it is not likely that they should have been taken from Celtic. Thus the word *bar*, Old English *barre*, French *barre*, Provençal, Italian, and Spanish *barra*, is found also in the Celtic language: Welsh and Armorican *bar*, *baren*, Irish and Gaelic *barra*; but I should hesitate to say that the French borrowed it from the Celtic. It is possible, indeed, that the other Romanic nations should have borrowed it from Celts living in their neighborhood: the Italians from the Celts of cisalpine Gaul, on their northern frontier; the Spanish from the mixed Celtiberian in the centre or northeast of the peninsula. But it is hardly probable that, acting independently, they should agree in taking the same word. It is possible, also, but not very probable, that the French (say) borrowed it from the Celtic, and then that the Italian and Spanish borrowed it from the French. We might raise the question whether the Celtic peoples did not borrow it from those of Romanic speech. Or, if this should seem inadmissible, we might fall back on the primitive relationship of Celtic and Latin (Italician) as members of the Indo-European family: and not only that, but members having (according to Schleicher's theory, which I believe is gaining strength with the progress of knowledge) a special connection with each other, closer than that which either of them has with any other branch of the family.

From these general remarks I come back to Koch and his treatment of the Celtic element in English. As already intimated, I cannot speak very highly of his method and results in dealing with the subject. In the first place, there is an extraordinary deficiency in the helps he used for his investigations. He gives a list of them on page 13, at the close of the chapter on Celtic words—a list which has at least the recommendation of brevity, for it consists of two titles only: the Dictionary of Scotch Gaelic, published by the Highland Society of Scotland, and Le Gonidec's Armorican or Breton-French Dictionary; both excellent and important works, it is true, but not the only important ones, nor the most important ones, for the end in view. Highland Scotch is only a dialect of Irish, a modern dialect, the monuments of which belong almost wholly to the last one hundred years. One who wishes a thorough knowledge of Gaelic must seek it in the Irish, which has a copious literature going back full a thousand years. The Armorican has had no direct influence on English; it has affected it only through French. It is, to be sure, very much like the Welsh, which has come into direct contact with English; and if we had no knowledge of Welsh itself, Armorican would be a valuable aid in determining whether the English was affected by the Welsh, and in what way or to what extent. But in fact there is no need of such aid, for Welsh is as fully known as Armorican, and has a much more copious literature, mediæval and modern, than that language. It is amazing that our author should have thought it unnecessary for these researches to contain anything about Welsh. Dictionaries and grammars of Welsh are easily procured; he could have had them without difficulty, if he had thought them necessary or important. This neglect of Welsh shows a very imperfect appreciation of the historic conditions. It must be remembered that, for centuries after the Anglo-Saxon conquest, native tribes of Kymric stock retained their independence and their language, not only in Wales, but also in Devonshire and Cornwall to the south, and in the so-called Strathclyde kingdom to the north. This kingdom included Cumberland on the western coast of England, and Ayrshire

in western Scotland, and reached north to the Frith of Clyde and the foot of the Scottish Highlands. The Strathclyde kingdom was not conquered till A. D. 912; it probably retained its Kymric language for some time later. Thus Anglo-Saxon, through nearly the whole Anglo-Saxon period, came in contact with Kymric peoples along its entire western frontier, for several hundred miles. In the southern part, indeed, in Devonshire and Cornwall, the native language was not Welsh itself, but Cornish, a somewhat different dialect of Kymric speech. With the Gaelic, during all this period, Anglo-Saxon came in contact only on the extreme north, and along a very short line at the foot of the Scottish Highlands. The influence of the Irish Gaelic could hardly be felt before the Irish conquest under Henry II., in 1172; and for a long time after that it must have been very inconsiderable. That words found in Anglo-Saxon or in the Semi-Saxon of Layamon and the Ormulum should have been borrowed from the Gaelic of Ireland or Scotland is not impossible, but it must be regarded in general as highly improbable. Yet Koch assumes this in the case of *clout*, A. S. *clût*, which he takes from Gaelic *clád*, 'patch'; in *cradle*, A. S. *cradol*, which he takes from Gaelic *creadhal*; and in *crock*, A. S. *crocca*, which he takes from Gaelic *crog*. With regard to none of these words does he attempt to show that they were found in Welsh or Cornish, or even in Armorican.

Another objection to Koch's method is that he hardly recognizes (at any rate, does not sufficiently recognize) the possibility that words found in Gaelic may have been borrowed from English. Yet such borrowing has demonstrably taken place, to a great extent. At one opening of O'Reilly's Irish Dictionary I find *baranta*, 'warrant,' *barboir*, 'barber,' *barbrog*, 'barberry,' *barcaim*, 'I embark,' *barun*, 'baron,' *batail*, 'battle'; in none of these cases does Koch assume the derivation of the English word from the Celtic; nor does Mahn in Webster's Dictionary; they are undoubtedly imported words in Irish. At another opening of the same book, I find *pillin*, *pilliun*, 'pillion,' *pilliur*, 'pillar,' *pillseir*, 'pilchard,' *pioraid*, 'pirate,' *pios*, 'piece,' *pes*, 'pease,' *pit*, 'pit.' Of these seven Koch

treats only one as originally Celtic: viz., *pillin*; but evidently this is only a variation of *pillun* (also given in O'Reilly), which in form shows itself to be simply English *pillion* spelt over into Irish. Here Koch's position seems the stranger, inasmuch as he appears to connect the word with Eng. *pillow*, A. S. *pylwe*; which, however, he identifies with Old High German *phuluwi*—and I might add that they all come from Lat. *pulvinus* or *pulvinar*. Koch derives Eng. *spigot* from Gaelic *spiocaid*; but close by this word in O'Reilly we find *spice*, 'spike,' *spid*, 'spite,' *spideal*, 'hospital,' *spiorad*, 'spirit.' The last three are found also in the dictionary of Scotch Gaelic; from which (to show how freely it has taken words from English, or Latin through English) we may quote almost at random examples like the following: *fasan*, 'fashion,' *feisd*, 'feast,' *fore*, 'fork,' *fortan*, 'fortune,' *fuirm*, 'form,' *fuirneis*, 'furnace,' *pearsa*, 'person,' *pinne*, 'pin,' *pinnt*, 'pint,' *plaigh*, 'plague,' *plannda*, 'plant,' *plasd*, 'plaster,' *port*, 'port,' *portair*, 'porter,' *post*, 'post' (in all senses), *priosanach*, 'prisoner,' *reubal*, 'rebel,' *companach*, 'companion,' etc., etc. There is no doubt that the words taken from English by Irish and Scotch Gaelic are far more numerous than those taken from Gaelic by English. Looking only at the numbers, in advance of special examination, the presumption is strong that a word borrowed by one from the other has been borrowed by Gaelic rather than by English. If the word stands by itself, without connection or explanation in English, and if the same word stands by itself in Gaelic, similarly without connection or explanation, the presumption is that the Gaelic took it from the English. This is not exactly the case with *spigot*; it is apparently connected with Eng. *spike* (Prov. Eng. *spick*), as if for *spicket*; and in like manner Gaelic *spiocaid* might be connected with *spice* (found, as just stated, not in the Dictionary of Scotch Gaelic, but in O'Reilly's Irish Dictionary). But the probability is that both *spice* and *spiocaid* were adopted into Gaelic from English. For *spike*, at least, Koch does not claim a Celtic origin. The word *pony* is perhaps unexplained in English (it may possibly be from *puny*, French *puisné*); the Irish *poni*, Scotch *ponaidh*, is equally unexplained in Gaelic;

but *poni* and *ponaidh* show by their spelling that they are modern importations into Gaelic, independent attempts in recent times to express an approximate sound to Eng. *pony*.

This suggests another objection to Koch's way of etymologizing from Celtic. He takes the Celtic word just as he finds it in the dictionary. He does not trouble himself about its history or connections; does not ask whether it has any history or connections in Celtic. But this is often a matter of much importance. If, on the one hand, the word stands alone in English, and nothing is found to explain it in the Teutonic or Romance languages; and if, on the other hand, the Celtic shows the root from which its meaning can be accounted for, or if the word appears at home in Celtic, among a family of kindred words that seem native to the soil, we should then be ready to acknowledge it as of Celtic origin. It is somewhat so, perhaps, with the word *to daub*; Irish Gaelic *dubh*, 'black,' *dob*, 'plaster, smut,' give a natural explanation of its meaning. The difficulty is that this word is as old as Wycliffe, and unlikely to come directly from the Gaelic; while the corresponding Welsh *du*, 'black,' *duo*, 'to blacken,' are without the final *b* of the root, and thus fail to account for the form of the English word. On the other hand, the Gaelic *righil*, 'dance,' is unexplained in Gaelic, but in our language is explained by connection with the verb *reel*. In all probability, it was borrowed by the Gaelic from the Lowland Scotch. It is not found in O'Reilly.

But a further objection, perhaps a more serious one, is that Koch refers to Celtic quite a number of words which occur in other Teutonic languages besides English, or in other Romanic languages besides French, and therefore, as already shown, are little likely to come from a Celtic source. Thus *pitcher*, which Koch refers to Gaelic *pige*, Armorican *picher*, is found (Mahn) in O. Fr. *picher*, *pichier*, *pechier*, Prov. *pichier*, *pechier*, O. Ital. *pecchero*, Mod. Ital. *bicchiere*, Span. and Port. *pichel*, Low Lat. *picarium*, *bicarium*; and Mahn traces it with much probability to O. H. G. *behar*, *bechar*, *pechar*, Icel. *bikar*, Eng. *beaker*. Again, *barter* is supposed by Koch to come from O. Fr. *barat*, 'cheating,' and that from Armor. *barrad*, 'cheating';

but Prov. has *baratar*, Ital. *barattare*, O. Span. *baratar*, Low Lat. *baratara*, all in the same sense of ‘cheating.’ Probably the Armorican word (for which I know no equivalent in other Celtic languages) is borrowed from the French. So *barnacle*, a kind of shell-fish or of goose, which Koch takes from Gaelic *barnach*, *bairneach*, and Armorican *brennik*, *brinnik*, through O. Prov. *bernatc*, reappears in Span. *bernacho*, Port. *bernaca*, *bernacha*, *bernicha*, Low Lat. *bernacula*, *bernicla*, *bernicha*, *bernaea*, *bernar*. Mahn thinks that the word was originally *hibernica* or *hibernicula* (sc. *lepas*, ‘shell,’ or *anas*, ‘goose’), because found in Ireland. The word is thus of Celtic origin, though in a different way from that which our author imagines. So *barrel*, which Koch considers as Celtic, received by us through Old French, is met with in Provençal, Italian, Spanish, Portuguese, and Low Latin; Mahn regards it as derived from *barra*, ‘bar,’ of which we have already spoken. So *bonnet* is found in Provençal, Spanish, and Portuguese, as well as French. Koch holds it for a Celtic word, because he finds a Gaelic *boineid*, although he has no Welsh or Armorican equivalent to show; how it came into French, he does not attempt to explain. The fact that the Gaels use it for a peculiar cap of their own seems to have weight with him—as if all other nations that have the word did not use it also for caps of their own’ For *chemise*, Koch is able to give an Armorican as well as a Gaelic word: viz., Arm. *kamps*, which is most likely borrowed from the French, with the same euphonic changes as O. Eng. *kemse* in Robert of Gloucester; the word runs through all the Romanic languages, French, Provençal, Spanish, Portuguese, Italian, and Low Latin, and is derived by Mahn from the Arabic *kamis*, ‘shirt.’ The words *dagger*, *gown*, and *harness*, which Koch thinks to have come from Celtic into French, and from French into English, have a similar diffusion among the Romanic languages, which makes their Celtic origin very doubtful. On the other hand, the word *marl*, ‘a kind of soil, a mixture of lime, clay, and sand,’ which he derives in the same way, is widely spread among Teutonic languages, being the same as O. H. G. *mergil*, Dutch, Dan., Sw. *mergel*, Icel. *murgill*. The word *pot* is still more widely

diffused: it is found in Celtic (Armorican, Welsh, Irish, Gaelic), but also in Romanic (French, Spanish, Provençal, Portuguese); and even in Teutonic (Low German, Dutch, Danish, Swedish, and Icelandic): though not in all independently, as is shown by the absence of the regular correspondence of mutes. With which it originated, it is hard to say: probably not with the Teutonic, where initial *p* is rare and is apt to be borrowed; perhaps not with the Celtic: at least, the Welsh *pot* is seemingly not ancient; if ancient, it would be *pod* in the modern language. Perhaps it is vulgar Latin, from the root *po*, 'drink' (the same as in *potus*, *poculum*), and borrowed by the Celtic and Teutonic languages.

I call attention further to a few words, in the order of their mention by Koch. For *clog* he gives Irish *clog* and Gaelic *cloguis*; but Irish *clog*, as given in O'Reilly, only means 'bell, clock,' and is probably the word *clock*, taken into Irish; and Gaelic *cloguis*, 'wooden block,' is probably our *clog*, taken into Gaelic. Mahn's etymology, which connects the English word with the Danish *klaeg*, 'sticky,' Icel. *kleggi*, 'compact mass,' is more plausible than Koch's. For *gag*, Koch compares Arm. *gak, gag*, Gaelic *gaig*, which signify 'stammering'; but feeling this not quite satisfactory, he brings in the Gaelic *gàg*, meaning 'cleft, split'; and, not content with this, he cites further Dutch *gagel*, 'gum,' and *gag(g)elen*, 'to squawk like a goose.' After these pretty desperate attempts at etymology, he suggests that the word may be an imitation of the noise made in choking; which is certainly far more probable than the previous explanations. He derives *crook* from the Celtic, though it is found in all the Scandinavian languages, and might most naturally have come into English through the great Danish invasions and settlements of the 9th, 10th, and 11th centuries. That the word *boast* should come from Celtic may be thought not improbable, as denoting a marked and prominent form of Celtic activity. But I am afraid we must own the borrowing here to have been the other way. Our English *boast* seems pretty well accounted for by words of similar sound which Mahn gives from German, Danish, and Swedish, meaning 'to swell' or 'to blow'; while the Welsh *host*, Ir. and Gae. *bosd*,

are suspiciously like each other and like the English word, and the absence of the word from Armorian is also an unfavorable circumstance. That *dainty* has any connection with Welsh *dantaid*, ‘toothsome,’ from *dant*, I find it very hard to believe; it is a case which stands too much by itself; if such borrowing had taken place at all, there would have been more of it. And besides, the sense of ‘curiously exquisite in form or finish’ (*dainty limbs, dainty carving*) is hardly likely to come from ‘toothsome, palatable.’

The word *slut* is referred by Koch to a Gaelic origin: what he would say of the Dutch and German words, similar in sound and sense, which Mahn gives, does not appear. Are they also derived from the Celtic? and if not, why must the English word be so?

A remarkable specimen of Koch’s method of etymologizing is afforded by the word *bucket*. In his Gaelic dictionary he found *buaid*, ‘bucket’; if he had looked further on in the same column, he would have seen *buesa* or *bocsa*, ‘box,’ *bucull*, ‘buckle,’ and in the next column *buideal*, ‘bottle.’ If he had consulted O'Reilly’s Irish Dictionary, he might have found *buicead*, ‘bucket,’ and in the same column *bucram*, ‘buckram,’ *bucla*, ‘buckle,’ and *buicleir*, ‘buckler.’ However, he only notices *buaid*, and never thinks of its being borrowed from English. He does not pretend to find any similar word in Welsh or Armorian; yet, meeting with Old French *buket* (Mahn does not give it, and I question its real existence), he is convinced that it must come from the Celtic, and with it our English *bucket*, though the latter might be naturally explained from Anglo-Saxon *būc*, ‘vessel, bucket, pitcher.’ And indeed, the Old French *buket*, if there be any such word, would with much more probability be referred to a Teutonic root like this, than to a Celtic word found only in Gaelic, and subject to strong suspicion of being borrowed from English.

Another strange piece of etymology is seen in connection with Middle Eng. *clapper*, ‘an inclosed place for rabbits to burrow in,’ from Fr. *clapier*. Now the word (as Koch himself says) is derived by Diez from Fr. *clapir*, ‘hide one’s self.’ But Koch does not scruple to derive it from Gaelic (also

Irish) *clab*, ‘lip, open lips, open mouth.’ He makes no pretense that the word occurs in Armoricæ or even in Welsh. There is only a slight relation in meaning, for the idea of yawning chasm seems foreign to the word under consideration. Yet Koch deliberately sets aside the natural and plausible etymology from the French itself, for an unsatisfactory one from Irish and Gaelic.

A still more amazing piece of etymology is the explanation of the word *tike*, ‘dog, cur.’ It is undoubtedly the same as the Old Norse word *tik*, ‘bitch.’ This word Koch regards as Celtic, though how it should come into the Old Norse he does not explain; perhaps in some piratical expedition against Britain they picked up the word for ‘house,’ and carried it home with them. For the Irish and Gaelic *tigh*, Welsh and Arm. *tŷ*, with which he identifies O. N. *tîk*, mean ‘house,’ and are the common Celtic words for ‘house.’ And in this sense he must suppose that it came into the Old Norse. For he imagines it got the meaning ‘dog’ from composition with *hund*, ‘hound’; *hund-tîk*, he thinks, meant originally ‘hound belonging to house,’ i. e. ‘house dog.’ Properly, however, if *tîk* meant ‘house,’ *hund-tîk* should mean ‘hound house,’ i. e. ‘dog-kennel,’ and not ‘house dog.’ This objection he does not notice, but assumes that the idea of house faded out, and that *tîk* was thought of as meaning ‘dog,’ and variously applied in this sense. Of the fact of its being feminine he seems not to be aware. How *tike* passed in English from the signification of ‘house’ to that of ‘dog,’ he does not attempt to explain, but contents himself with saying that it was applied to designate objects of very different kinds, but all belonging to the house, as *peasant, clown, young cattle, old horse, wretched dog*, etc. It probably would be safe to say that all the objects it was used to designate were such as might be thought to deserve in some way the epithet of “dog.”

I will not, however, pursue these criticisms further, though I have by no means exhausted all that seems to me objectionable. I think, in fact, that a very large part of Koch’s Celtic etymologies are either certainly or probably wrong. Very few of them appear to me certainly right, except those words

which I mentioned at the outset, words which denote Celtic objects or activities, such as *bard*, *brogue*, *clan*, *druid*, *plaid*, *shamrock*, *whiskey*; to which class belong also *bannock*, ‘round cake of oatmeal,’ *fillibeg*, ‘kind of short coat,’ and *pibroch*, ‘martial music by bagpipe.’ As most probable, next to these, I should mention the derivation of *glen* (referred to already), *bog*, ‘morass,’ *brock*, ‘badger,’ *yarran*, ‘nag, hack’; and in less degree, *bodkin*, *dirk*, *loop*, *mug*, *noggin*, ‘small mug,’ *peel*, ‘small fort,’ *piggin*, ‘small wooden dipper.’ The Celtic derivation seems probable also for the following, which come through the French: *basket*, *bran*, *brisket*, ‘breast of animals,’ *button*, *car* and *cart*, and *comb*, ‘valley.’ *Breeches* also we know to be Celtic, though it comes through the Latin and Anglo-Saxon. The whole number of articles, i. e. distinct etymologies from Celtic, given by our author is little less than a hundred. Those which strike me as certain or as having much probability are hardly more than one-third of that number.

I am strongly impressed with one conviction: that no man can be really fitted for investigations such as Koch undertook, unless he has such command of the Celtic language that he can tell what is genuine Celtic, and what borrowed by Celts from English or French or Latin. The number of such borrowed words in the vocabularies of all living Celtic languages—and of Cornish, too, which is not living—is very large. Unless etymologists can distinguish these, they will be liable to constant mistakes. I am very glad to see from an article by Mr. John Rhŷs—a Welshman, I suppose, and an Oxford scholar, who has lately spent some time in Germany—that Windisch is studying Celtic. The statement is that he is preparing himself for a place in the University of Leipsic, as Professor of Celtic. Some will remember an elaborate and very able article of Windisch, lately published, on Indo-European relatives. He is a pupil of Georg Curtius, thoroughly trained in comparative philology, a young scholar of excellent abilities and great promise. He will do a good work, I hope, for a field of study which sorely needs good workers.\*

[\* Windisch has contributed the Celtic etymologies to the fourth edition of Curtius's *Grundzüge der griechischen Etymologie*, published in the summer of 1873.]

IV.—*On the Pronunciation of Latin, as presented in several recent Grammars.*

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The demand for a philologic—that is to say, a Roman pronunciation of Latin—has become so strong, that we are at length provided with grammars which follow the ancient authorities as far as modern habits allow.\* The most important and the most carefully prepared of these grammars, is that of H. J. Roby, who rigidly excludes mere ‘English’ and ‘Continental’ empiric practice, for the ancient Roman or true powers. Along with Bentley, Donaldson, Key, Payne Knight, John Jackson,† Chavée, Eichhoff, Rapp, and Noali Webster, he regards ‘v’ as English ‘w’ (way), and in some cases he writes ‘v’ after ‘q’ (as in ‘qvinque’ p. 38, ‘qvinqve’ p. 334), which is correct when ‘v’ and ‘u’ are separated for consonant and vowel. His adoption of Italian ‘o aperto’ disturbs the law of interchange with ‘u’ (oo), as in the passage from ‘oinus’ and ‘œnus’ to ‘unus,’ where, before the elision of the close ‘e,’ it aided in closing o to u; and ‘e’ as in thère, mět (instead of thēy, wěight), offends Spanish, and gives a forced Greek analogy. Of course ‘c’ (cay) and ‘g’ (gay) are assigned their power in *kin* and *get*; and it is inferred, without ancient authority, that ‘s’ between vowels ‘at one time probably’ had a second sound like that in *rose*. But while Italian ‘rosa’ has this sound, ‘così’ has true s, and Spanish has the hissing s alone. In places where it might have had a tendency to develop a sonant phase, it was either elided, as in TRE-DECIM, or it became the cognate liquid r, as in ROS, RORIS.

Dr. Donaldson had spoken dogmatically of φ as p'h, a

\* For example, those who wish to find Norman-English v in Latin, look for it in words with ‘v,’ rather than in those with ‘b’; and others, in their endeavor to associate the diphthong of boy (instead of bō-y) with ‘œ,’ turn ‘o’ into œœ.

† *Chronological Antiquities*, London, 1752.

post-aspirate of *p*, and Mr. Roby (p. 19) adopts this view, assigning *p'h*, *t'h*, *k'h*, to Greek—a view which does not account for transcriptions like ‘Phichol,’ ‘Ruth’ (or *ρούθ*), ‘Malachi,’ and Φάβιος.

In 1851\* I stated my belief that certain Greek and Latin words are older than their Sanscrit cognates, and I doubt the relative antiquity of the Hindoo post-aspirates—*p* being probably older than *p'h*, *ph.* and *f.*† I had previously‡ determined *φ* to be an *f* made with the lips alone, and related to German ‘w,’ as *p* to *b*—a view which was confirmed by Castanis,§ and by E. A. Sophocles.||

An aspirate (such as *h*, *f*, *s*, *th*,) may arise after another consonant, from several causes. As the vocal cords are not approximate and parallel when the surds (*p*, *t*, *f*, etc.) are made, if they are not set simultaneously with the organs to form a subsequent vowel, an intended *pâ* will be represented by *p'hâ*; but if the lips lag, or if the tongue interrupts, then *fâ* (*PATER, father*), or a German *pfâ* (*PONDUS, pfund*) or a Greek *psâ* (*PULEX, ψύλλα*) may result. In these cases, carelessness, or want of energy produces these weakenings, but energy of speech will have the same effect, as in English, where an energetic ‘ca’n’t!’ becomes ‘k’hant!’ About the year 1850, the illiterate of the city of New York had ‘b’hoy’ as a form of *boy*, but many of them could not accomplish it without an obscure vowel between the *b* and *h*.

For physiologic reasons, *h* is a difficult consonant to make, and in ‘*p'h*’ it would be got rid of by elision, that is, by dropping it; or an otosis would cause it to unite with the *p* to form *ph* or *f*, and we find that even in parts of India, ‘*fal*’ is heard for ‘*p'hal*’ (fruit).

\* Elements of Latin Pronunciation, pp. 11, 63.

† The fact that Sanscrit *p'h*, etc., are represented by single letters, is against them as original sounds, like *ts* in German, *tsh* in Italian, and *dzh* in English and Arabic. The presence of *tsh* and *dzh* in languages, is against their phonetic antiquity, and so-called “roots” with these compounds, and with Greek *ζ*, are seldom genuine.

‡ Phonetic Advocate, Dec. 1849, p. 100; Proceed. Am. Acad. 1849, p. 171; Analytic Orthography, p. 32.

§ The Greek Exile, Philad. 1854, p. 246.

|| Greek Alphabet, 2d ed., 1854, pp. 113, 114.

This takes place in Greek compounds like ‘aphæresis’ for ap’haeresis, and ‘anthelmintic.’ Had ‘ant’helminthic’ been possible, we might have had other dental post-aspirates, but we find the *h* elided after the fluent sounds *l*, *n*, as in Φιλιππος for φιλιππος; and in ἄν-νδρος, where the *h* of ὥδωρ would seem as practicable as in the English form *anhydrous*. In ξάρθ-ιππος and ἀρχ-ιερεύς, the *θ* and *χ* belong to the initial word. The *h* is elided, even where it might prevent hiatus, as in πολύνδρος, πολυίστωρ, ἄϋπνος; while, according to the Donaldson theory, it has two places in φθίσις, and in χθών. Rhetor for ρήτωρ, is an offense to Welsh.

The lisp of *th* shows its relation to *s* in forms like Θεός and Σιός, Spanish ‘zandalos’ and ‘sandalo,’ and the post-aspirate theory is farther contradicted by *ξ* and *ψ*, the former of which might represent *χc*, and the latter *φc*, which would bring *h* before *s*.

In Bingham’s Grammar (Philadelphia, 1867) the ‘Continental’ precedes the ‘English’ method, with a power like English “short *a* as in *hat*”—a rare sound which is scarcely known on the European continent—and the same may be said of his “*eu* in *feud*,” which is not even a diphthong. The proper short correlative of the *â* of *fâr*, *fâther*, is that of *ärt*; Latin and Italian *î* of *marîne* is short in *rëcept*; and *ô* is short in *ö-bey* (*not* being the short of *nought*). If *æ* (ái) and *œ* (öy) are turned into ‘*a*’ in *made*, they are vowels, and cannot be subject to diæresis.

In Bartholomew’s Grammar (Cincinnati and New York, 1873) the ‘Roman Method’ is adopted, the ‘English Method’ being sent to the appendix. Two of the vowels (e, i,) have differing long and short powers (prey, pet, and ravine, ratify,) and ‘*z*’ is given as *ds* or *sd*. The descriptions of the ancients, and the structure of Greek words, show that ‘*z*’ was equivalent to English *zd*, and the attempt to make it ‘*ds*’ tends to vitiate the laws of etymology. Tory (b. ab. 1480, d. 1533) asserts that at Bourges (his native place) the name of the letter ‘*z*’ was *easd*, and he says that ‘for *gaza* the ancients pronounced, and often wrote *gasda*.<sup>\*</sup> Notwithstanding several minor points, Mr. Bartholomew’s pronunciation is very nearly that of the ancient grammarians.

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\* Quoted in Jaubert’s *Glossaire du Centre de la France*, 1864, p. 708.

V.—*On some Points in the Life of Thucydides.*

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Of the life of Thucydides very little is known upon really good evidence, but that little rests upon the very best of testimony, his own. He tells us incidentally a few facts about himself, in very much the same way in which Herodotus does, and Xenophon. Like Herodotus, he begins his work with a sort of title or avowal of authorship, in which he gives the name of his native city, and occasionally he describes some event or place in the language of one who had himself seen it. Like Xenophon, he had himself a part in the history he narrates and his action appears in its proper place in the narrative. In regard to the principles and purposes of his history he is more full in his statement, as well as more free in his criticism of other writers, than either Herodotus or Xenophon, but in regard to his own life what information he gives is more scanty than what we learn about themselves from the other two great historians.

We should naturally then turn to other contemporary, or nearly contemporary, writers for some addition to our knowledge. There would seem to be plenty of them, for the time of his life and the century following is the richest period of Greek classical literature. All the comedies of Aristophanes, with their abundant details about the most trifling events and obscure men, belong to the last thirty years of the fifth century B. C. Then comes Plato with his thirty-six dialogues. As reports of the conversations of Socrates they ought to be full of local coloring and references to every-day life, and so we find them. Plato quotes from or mentions all the principal poets—for instance, Orpheus, Musaeus, Homer, Hesiod, Tyrtaeus, Simonides, Anakreon, Solon, Sophokles, Euripides. Naturally he refers to the philosophers who preceded his great master, but also to Hippokrates, the physician,

and Phidias, the artist. And of the men of history he refers to Miltiades, Themistokles, Kimon, Perikles, Thucydides, son of Melesias, Brasidas, Laches, Nicias, and others. Still he does not mention the name of Herodotus, nor is there any reference to Xenophon, his fellow pupil in the Sokratic school. Then comes Xenophon with a continuation of Thucydides' history and other historical and miscellaneous writings ; but they show a rather remarkable lack of references to the events and literature of the century before his own life. After him comes the period of Demosthenes, with whom we include all the orators, whose works are full of references to past history. They bring us down to the reign of Alexander, and there we have a most voluminous writer, a sort of encyclopædia in himself, Aristotle. Now in all these writers of the time of Thucydides and the century following his death, we should certainly expect to find some reference to Thucydides, some quotation from his work or information as to his part in the history, for he was not a mere writer, but a man of affairs and a general of the army. It is remarkable that no one of them all, not even Xenophon, whose history seems designed as a continuation of his, ever once mentions his name or alludes to him in any way. Even with all allowance made for the fact that such references to one another, as authorities or in criticism, are comparatively rare in the writers of that original creative era, it seems strange that one who had written a work so fitted to be an authority for its time, so full of matter in both narrative and reflection, and who had also taken a position by no means obscure among his countrymen and the people of other states, should never be referred to by historian, philosopher, poet, or orator. One might almost think that the guild of literary men, of writers and speakers, had taken offense at him because he had so completely ignored their fellow-craftsmen of the period of which his history treats, and had chosen this method of revenge. For it is one of the notable things about his work that it contains no reference of any kind to the great writers of his century, unless, indeed, the rhetorician Antiphon be counted among them. Neither Pindar nor Hesiod, neither Aeschylus nor Euripides, nor

Sophokles, nor Aristophanes, nor any of their rivals on the Dionysiac stage, is so much as mentioned by Thucydides. Nor does he give a word to Sokrates or any of the professed teachers of the art of reasoning, nor to any of the sculptors, painters, architects or musicians of his day. The great works of Iktinus and Phidias on the Akropolis he refers to only as having diminished by their cost the accumulated fund with which the Athenians entered upon the war with the Peloponnesians. This silence of his we may understand by noticing the avowed object of his work, viz., political and historical instruction. This he understood to mean the lessons drawn from the movements of communities of men in their relations to one another, and while he recognizes in the words he puts into the mouth of Perikles in the funeral oration, as well as elsewhere, the influence of amusements and visible manifestations of a nation's power and genius in forming the nation's character, yet his own intellectual taste led him to the study of such character as shown in negotiations, conflicts, and party movements. His strict adherence to his object thus defined was the indulgence of that strong taste.

For the silence of others in regard to him, several partial and possible causes may be suggested. The disastrous close of the Peloponnesian war may have made the Athenian writers and speakers, always boastful of their country and always blind to an unpleasant truth, disinclined to refer to that period of their history. As their ancestors censured Phrynicus for his tragedy of the Capture of Miletus, but applauded the Persae of Aeschylus, so they would refer back to the Persian wars and to the period of Kimon and Perikles, rather than to the later time of their misfortunes. Certainly the references to events of the Peloponnesian war are comparatively rare. Yet any Athenian might without shame have quoted from the funeral oration of Perikles, or boasted of the naval victories of Phormion, one would think.

Perhaps the aristocratic leanings of Thucydides and the unsparing impartiality of his history may have made him an unpopular author in the time of the democracy and while the hatred engendered by defeat was still strong in the minds of

the people. Yet his aristocratic politics did not prevent him from doing justice to the times of democratic supremacy at Athens (as notably where he describes the state of things after the overthrow of the Four Hundred, viii. 92), and certainly in the time of Aristotle, if not quite in that of Demosthenes, the bitterness of hatred against the old enemy must have faded away from most minds.

The style of Thucydides, especially in the speeches, and the general absence of any attempt to make his work entertaining by myths and digressions, may have made it unpopular among readers, and hence little known in the succeeding generations. Yet it must have been known to Xenophon if, as is supposed, his *Hellenika* was written as a continuation of it; we are told that it was transcribed eight times with his own hand by Demosthenes, and we can hardly doubt that it was regarded as a work essential to all thorough study of history and politics, even if not advertised as "a book which no family should be without." These suggested causes may have contributed to the result, yet all together they hardly explain it, and in my view it remains a sort of puzzle why we find no reference to Thucydides in any writer whose works are preserved to our time until we come down to Dionysius of Halikarnassus in the last century before the Christian era. Then there are a few stories about him in some writers of the first and second centuries, and a brief notice in Suidas. The two lives, one without name, the other under the entirely unknown name of Marcellinus, both of uncertain date, I leave out of view entirely, because the uncertainty of their date and authorship makes them worthless as authorities, and they prove themselves to be uncritical and untrustworthy. In the nature of the case no certain knowledge can be gained from these later writers, unless they refer to contemporary authors or records preserved to their own time. From their own knowledge they can tell us nothing—and what they may tell from tradition is worthless unless there is some authority for the tradition.

Let us then consider what the historian tells us about himself. He tells us that he was an Athenian, that his

father's name was Olorus; that he was sick with the plague which was so fatal at Athens during the second and third years of the war, that he owned or worked gold mines in Thrace, which fact gave him influence among the people of the neighborhood, that he was sent out as general with one of his colleagues to conduct the military operations on the Thracian coast in 424 B. C., that the failure to prevent the capture of Amphipolis by Brasidas led to his being an exile from his home for twenty years, and that he devoted himself during that time, as with more restricted opportunities he had done from the beginning of the war, to collecting materials, visiting scenes, and sifting evidence for his great work, the history of the Peloponnesian war. This is the whole of our actual knowledge about his life, conveyed in four well-known passages in the 1st, 2d, 4th and 5th books of the history. There is nothing else from the date of his birth to the date of his death that rests on so good evidence, nothing else that has not been disputed and differently understood by different persons. We may draw some inferences from these statements of his, but they do not carry us much farther than the statements themselves. That he was a man somewhat prominent in the public life of the state, may be inferred from his being elected one of the ten generals for the year 424, yet we know of no other office that he held. That he was a man of independent means, may be inferred from his being able to devote himself for life to the labor of writing this history, and may perhaps be explained from his having the contract to work the Athenian gold mines in Thrace. That he belonged to the moderate aristocracy, the party of Perikles during his life-time, may be inferred from his evident admiration of that great man and from scattered passages in his work.

In one case we may perhaps safely combine his statements about himself with what is known from him and from other sources, so as to explain a point which he somewhat remarkably leaves unexplained. In reference to that long absence of his from his native land, he uses a singularly guarded and unsatisfying expression. Instead of telling exactly in what

form that absence occurred, whether it was a voluntary or an involuntary exile, he says merely ξυνέβη μοι φεύγειν τὴν ἐμαντοῦ ἔτη εἰκοσι μετὰ τὴν ἑς Ἀμφίπολιν στρατηγίαν. This expression leaves it entirely uncertain whether he was banished by vote of the people, or was condemned to death as a traitor and staid away to avoid the penalty, or simply imposed upon himself the long exile from unwillingness to face the natural indignation which the people at home would feel towards an unfortunate general. Each of these views has been maintained, and there is no clear and positive argument to make us decide for either of them. There appears, however, some reason to believe that the last supposition is not correct, that his exile was not wholly voluntary, but was caused by some action of the government, in the apparent coincidence of its beginning and end with variations in the state of parties at Athens. He was general, it appears, for the year 424, that is for the year beginning in June, 424, and ending in June, 423. It was in the late autumn or winter of that year (in November, Classen says), that Brasidas surprised Amphipolis, and the twenty years of exile probably began at once. If the historian's words, ἔτη εἰκοσι, are to be taken exactly, and there is no reason to qualify them, the exile would end in the latter part of 404. Now the first date falls at the time of Kleon's greatest power, just after his utmost success, the capture of Sphakteria, and about three years before his death at Amphipolis. It was a time of democratic ascendancy. Kleon stood almost alone against Nicias and Demosthenes, carrying the assembly with him and governing Athens almost as Perikles had during the later years of his life. I do not mean to assume, in the absence of any evidence, that Kleon himself was active in procuring the banishment of Thucydides, nor would I in that way account, as Grote does, for a supposed bitterness of the historian against the demagogue. The one phrase upon which Grote mainly relies as evidence of that bitterness, where Thucydides speaks of the proposition of Kleon to take command of the army himself against the Spartans in Sphakteria, as "an insane proposition," seems to be nothing more than what any writer might justly use. It

was an insane proposition, even when military science was not so far advanced as it is now, or indeed as it was after the improvements of Epaminondas and of Alexander, for a civilian who had seen no military service to offer to take command of the army in the field. It was especially so at this critical moment when the Athenians had a most unexpected and unparalleled prize just within their grasp, but not yet by any means beyond the possibility of its slipping away. As Grote himself points out, the proposition was not made in earnest by Kleon himself, and his success in fulfilling his inconsiderate promise was due to his unwonted wisdom in leaving the actual management of the troops entirely to Demosthenes—so that neither a sober judgment of Kleon's beforehand nor the successful issue afterwards can relieve the proposition from the censure of being insanely rash, nor can such an opinion of it be ascribed to personal hostility on the part of Thucydides. We may reasonably suppose a sentence of banishment or death to have been passed upon him in the supremacy of the popular party, without supposing Kleon to have been prominent in bringing it about. The second date, on the other hand, 404, if we are right in taking the words *ἐν τικοῖ* strictly, falls in the time immediately after the capture of Athens by Lysander, when the oligarchy of the Thirty was in power. Many exiled members of the aristocratic party returned to Athens then. Now it seems natural to connect together the difference between the parties dominant at Athens at these two points of time with the change in the relations of Thucydides to the government, and to suppose that he was in some form subjected to penalty by the popular majority in the assembly, and returned to Athens only when the change in political affairs made home promise safety to him. We do not need to impute to him sympathy with the outrageous excesses of the Thirty. He may soon have left Athens on account of those outrages, as Classen supposes, and returned to his Thracian possessions, a supposition which would explain the tradition of his having died in Thrace.

There is one other point in relation to the life of Thucydides upon which some light is thrown by a combination of his own

statements with what is furnished by other writers, some of them of a much later date. The point I refer to is the family to which the historian belonged. As has been already mentioned, he tells us but little, yet that little, of course, is itself beyond question, and furnishes a firm ground on which the inquiry may proceed. The first two words of his history, Θουκυδίης Ἀθηναῖος, suggest that he was by birth an Athenian citizen—and that this is not an unwarranted inference may be seen from the fact mentioned in the fourth and fifth books that he was a member of the government as general, an office to which at that period none but citizens were admitted. Also in the fourth book (§ 104) he calls himself Θουκυδίηρ τὸν Ὀλόρου, thus giving the name of his father, in the common form of particular designation of an individual person. Thus is he distinguished from Thucydides, son of Melesias, a more prominent contemporary, and from others of the name. These are the principal facts known upon his own testimony, and now let us see how they can be brought into connection with information drawn from other sources.

The first thing to be noticed is the name of the father, *Olorus*. It is not a common name. It occurs nowhere else in Greek literature, so far as I can ascertain, except in two passages in Herodotus, vi. 39, 41. There it occurs as the name of a king of the Thracians, apparently of those in the neighborhood of the Chersonnesus, whose daughter Hegesipyle was taken in marriage by Miltiades. This at once naturally turns our thoughts toward Thrace and the family to which Miltiades belonged, to find further information as to the name Olorus. The connection of that family with that country was a long and brilliant one. In the reign of Peisistratus at Athens, about 550 B. C., a Miltiades, called the first of the name, became tyrant of the Thracian Chersonnesus, at the command of the oracle at Delphi. He was honored by the people of the Chersonnesus with the sacrifices and games customary for an oekist or founder of a community, which apparently were still kept up in the time of Herodotus (vi. 38). He was succeeded at his death by Stesagoras, a son of his half brother Kimon, which Kimon is also the first of his

name known to us. On the death of Stesagoras, his brother, a second Miltiades, the one who made the name famous, was sent out by the sons of Peisistratus, who were now in power at Athens, to take the sovereignty in the Chersonnesus. Here he established himself by treachery and with a force of mercenaries, taking also the daughter of Olorus as wife, apparently to strengthen himself by the alliance. Olorus is mentioned simply as “the king of the Thracians” *τοῦ Θρηικῶν βασιλέος*, without any limitation, so that neither his residence nor the extent of his dominion is known. This establishment of a tyranny by Miltiades the Great must have taken place before the overthrow of the Peisistratidæ in Athens, that is before 510 B. C., and he was obliged finally to abandon his government there by the approach of the Phœnician fleet of Darius in 495 B. C. After him comes his famous son Kimon, not indeed as successor in his tyranny in the Chersonnesus, but as general of the Athenian republic, active and successful in extending its power over all the debatable regions between Greece and the Persian empire. He conquered Eion, opening for colonization the site of Amphipolis, Scyros, bringing home the bones of Theseus, and Thasos, securing to Athens the revenue of the gold mines on that island and on the opposite mainland. He expelled the Persians from the Chersonnesus and made it part of the Athenian domain. Thus we see the principal activity of the lives of both Miltiades the Great and Kimon to have had for its scene the coast of Thrace and the adjacent islands, where also the first Miltiades and Stesagoras, his nephew, had held power. For nearly a century, from 550 to 460 B. C., some one of this family was always either tyrant in the Chersonnesus or fighting in that region to establish his own or his country’s control over it. These statements rest on the authority of Herodotus and Thucydides.

Here you will perhaps ask, as the writer of the life of Thucydides ascribed to Marcellinus supposes a reader of his own work to ask in reference to the genealogy of Miltiades which he gives, *τι αὐτῷ πρὸς Θουκυδίδην*; what has all this to do with Thucydides? Instead of answering as he does at once “They were connected in family,” let me rather set

before you the facts in order and then suggest the inference. Thucydides tells us (in iv. 104) that he was sent out with one of his colleagues in the generalship to conduct the operations of war in the region of Thrace, and was himself at Thasos when the news came to him that Brasidas was advancing upon Amphipolis, where the other general was. In the next section he says that Brasidas was moved to the greatest haste in his effort to get possession of Amphipolis by the fear of aid to the Athenian garrison from Thasos, and by learning that Thucydides who was there had control of the working of the gold mines in that part of Thrace, and from that fact had great influence among the people of the neighborhood. The phrase used here seems to have been inaccurately construed by most writers upon the life of Thucydides from the earliest down to modern times. Plutarch first in his life of Kimon (§ 4) changes it into *τὰ χρυσεῖα περὶ τὴν Θράκην ἔκειτο*. Marcellinus, whoever he was, uses a similar phrase and adds a tradition that Thucydides acquired the property by marriage with a Thracian heiress. So even Böckh in his *Staatshaushaltung der Athener* says that Thucydides "*Goldgruben in Thrake besass,*" and Grote (vi., p. 409) speaks of him as "a large proprietor and worker of gold mines." Now the words of Thucydides, our only real authority, are *πυνθανόμενος* (ό Βρασίδας) *τὸν Θουκυδίδην κτῆσιν τε ἔχειν τῶν χρυσείων μετάλλων ἐργασίας* ἐν τῇ περὶ ταῦτα Θράκη καὶ ἀτ' αὐτοῦ δύνασθαι ἐν τοῖς πρώτοις τῶν ἡπειρωτῶν. The word *ἐργασίας* here seems to require beyond question the interpretation that what he owned was simply the right of working the mines, not the mines themselves, that he held a contract from the owner, that is the state of Athens, to work them. With this agrees the plural *τῶν χρυσείων μετάλλων*, for though the singular is sometimes used collectively as for the whole work of a district containing several mines, it does not appear that the plural is ever used of one mine. He could hardly have been owner of all the mines in that part of Thrace, for they constituted an important part of the property of a state in the possession of Thasos first and afterwards of Athens. With this understanding of the matter then we leave entirely out of view the discussion upon which Böckh pronounces an opinion, and in

which Krüger in his life of Thucydides engages, as to how these mines came into the possession of the historian. We hold that he was not the owner by inheritance or by marriage, but simply the contractor under the Athenian government, which leased the use of the gold mines in Thrace apparently as it did that of the silver mines at Laurium. Such a contract might come into a man's possession by inheritance or by purchase, and how it was in the case of Thucydides we have no means of deciding. Furthermore the position of contractor for the working of a district of mines serves to account for his commanding influence among the chief men of the population on the mainland as the ownership of a single mine or two would not account for it. We find the historian then in his day and generation in connection with this Thracian coast, as the family of Miltiades and Kimon had been for nearly a hundred years in the preceding generations. We find as the name of his father the same which occurs as that of the father-in-law of Miltiades some fifty years before the probable date of the birth of Thucydides—a name too which occurs nowhere else in Greek literature and sounds foreign to the Greek language—a name which has no apparent etymological connection with any Greek stem, whereas the Greek proper names of persons are generally easy of explanation. Curtius indeed suggests (Gr. Etym., p. 312) a possible connection with the stem of ὄρνις, but points out that the form resembles more nearly the Slavonic kindred word, *oră-lŭ*, ‘an eagle,’ than the Greek forms of the stem.

These things suggest a family connection between Thucydides and Miltiades or his descendants. Now we find that this family connection was believed in by the Greeks of the time of Plutarch, and he adds a fact which he regarded as confirming it; that a tomb or monument of Thucydides ἐν τοῖς Κιμωνείοις δείκνυται παρὰ τὸν Ἐλπινίκης τῆς Κιμωνος ἀδελφῆς τάφον. The expression seems to indicate that he had himself been on the spot and seen the tombs, and it is good authority for a current belief at Athens in the first century of our era. Pausanias, too, a little later than Plutarch, speaks of a monument (*μνῆμα*) to Thucydides in Athens near the Melitid gate, but this can hardly be cited as confirmatory evidence, for he

says nothing as to its being in connection with other tombs, and there is no good evidence to show where the Melitid gate was situated. If it could only be proved that this gate opened upon the *Κοιλη* or Valley road, where Herodotus tells us that Kimon was buried, then the evidence of Pausanias would be a most valuable completing link—but without that knowledge we have only Plutarch to rely upon. In his next sentence he goes on ἀλλὰ Θουκυδίδης μὲν Ἀλιμούσιος γέγονε τῷρες δῆμων, οἱ δὲ περὶ τὸν Μελτιάδην Λακιάδαι. Whence he got this latter statement does not appear, nor is the former sufficiently explained by citing the inscription Θουκυδίδης Ὁλόρον Ἀλιμούσιος ἐνθάδε κεῖται—for that inscription is found only in the lives which we have seen are wholly valueless as authorities. Plutarch may have seen such an inscription, but there is no evidence that he did. Yet that contradiction between the demes of Thucydides and Miltiades, even if we allow both to be known on good historic evidence, does not seem to be fatal to a family connection between the two men. For there is no reason to doubt that the deme membership followed the male line, and so a sister's son to Kimon would have been in the deme of his father, Kimon's brother-in-law, not in that of Kimon himself. However that may be, the fact attested by Plutarch remains, that there was in Athens a group of tombs and monuments called those of Kimon's family, bearing their names probably as modern discoveries show was the practice, and that among them was one assigned to Thucydides.

Here then is the argument for connecting the historian in some way with that well-known family. His father's name was Olorus, a name which occurs nowhere else but as that of the father of Miltiades' wife, Kimon's mother. He himself had the working of the Thracian mines opposite to Thasos, and on account of his local influence was sent thither in his year of generalship, to the same region where Miltiades and Kimon, following the lead of others of the family before them, had been active in government and in war. A tomb among those of the same family was pointed out as his in the time of Plutarch. It seems to me that the coincidence of these three facts in any other case would suffice to establish reasonable ground of belief in a connection of blood between the man

and the family. What precisely the connection was, we have of course no means of ascertaining. Any statement, such as that of Marcellinus, must be a mere guess, but we may be allowed to frame such an explanation in order to remove any doubt of its possibility. Miltiades, we know from Herodotus (vi. 41), had sons besides Kimon by other women besides Hegesipyle. He may have had others by her whose relation to him is not mentioned in our scanty records. One such may have been this Olorus, named from his mother's father as Kimon was from his father's father. This Olorus may have married an Athenian wife and so have given full citizenship to his son Thucydides. Yet by the law of Pericles, passed about 460 B. C., all persons born of mixed marriages, as between an Athenian man and a non-Athenian woman, were struck off from the lists of citizens. This, being retro-active, would have excluded both Olorus and his son. Besides, the difference of deme mentioned by Plutarch, if we accept his testimony, would prevent such direct connection. There are other possibilities that avoid these objections. A sister or a daughter of Miltiades may have been married to a son of Olorus, king of the Thracians. A son of theirs, born perhaps about 500 B. C., and named after his paternal grandfather according to Greek custom, may have been naturalized as a citizen at Athens by virtue of his mother's citizenship, and adopted into the deme Halimus. Then taking an Athenian wife he may have been the father of Thucydides, the earliest and in my view most probable date of whose birth is 471 B. C.

The name *Thucydides* indicates nothing, for it is one which might be given from a mere fancy for it, with or without any family reason. Often in Greek families there appears a custom of naming the first born son after his paternal grandfather, so that the same two names alternate in successive generations. But there are also many examples of names, some of them even like this having patronymic form, which have no family significance, but seem to have been chosen from their meaning as words or their popularity as names. This name appears to be a patronymic from Θεοκνήτης, which appears only in one instance in Herodotus in Ionic form and in the genitive case, Θεοκνήτους.

VI.—*On the Classification of Conditional Sentences in Greek Syntax.*

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More than thirteen years ago I called attention to some serious difficulties which seemed to me to beset the common theories of conditional sentences in Greek syntax, difficulties which extended in a less degree to Latin and even to English syntax. As the remedy then proposed was a radical one, involving the abandonment of many generally accepted doctrines, as well as a reconstruction of the classification in its most important parts, it cannot be amiss to review the whole question in the light of later experience, that we may determine, if possible, what system of classification best represents the present state of grammatical science. It is to be hoped that very few scholars, if any, still hold to the antiquated notion that grammar is not a progressive science, and that all its important principles have been handed down to us from some infallible authority in past generations. Such a doctrine would bring upon classical studies most deservedly the reproach which some popular writers ignorantly cast upon them, that of remaining stationary and refusing to recognize new truth and to be governed by scientific principles in a scientific age. Until the generation has passed away which can remember Porson's controversy with Hermann about the common rules of iambic verse, surely no one can be charged with impertinence for suggesting doubts as to the correctness of any generally accepted principle in Greek or Latin grammar.

The question which goes to the root of the whole discussion of conditional sentences is one which every schoolboy is taught to answer at a very early stage in his classical studies,—What is the essential force of the Greek subjunctive in protasis as opposed to the simple indicative, e. g. of *ἐὰν πράσσῃ τοῦτο* as opposed to *εἰ πράσσει τοῦτο*? The various answers to this elementary question exhibit in the strongest light the vagueness and looseness of much of the common reasoning on the

whole subject. Most grammarians agree in assigning to the subjunctive the idea of "possibility" with various modifications. The definitions "possibility with prospect of decision,"—"objective possibility,"—"what is possible now or in the future,"—"bedingte Möglichkeit,"—"eine Tendenz zur Wirklichkeit," etc., are familiar to all scholars, and most of us have probably learnt and repeated one or more of them in the belief that they really contained the essence of the subjunctive in protasis. But what mind accustomed to the exactness of modern scientific definitions can rest satisfied with any such vague formula, when it professes to include two such dissimilar expressions as *ἴαν ἐλθῃ, τοῦτο ποιήσω*, and *ἢν οὐγγὺς ἐλθῃ θάνατος, οὐδεὶς βούλεται θυνῆσκειν?* A pupil who has brought his mind to accept such definitions, in a science which professes above all things to teach exactness of thought and expression, cannot be very strongly impressed with the boasted accuracy of Greek in expressing nice distinctions. It surely cannot require much reflection to see that, whether "possibility" or "tendency to reality" is, or is not, an essential part of these two expressions, there is a great deal involved in both of them which no one of the common definitions attempts to touch. All omission of the important matter of time, or the introduction of it by "now or hereafter," is, to say the least, a marked defect. An enquiring mind might perhaps notice that *ἴαν τοῦτο πράσσῃ* sometimes means *if he shall do this*, and sometimes *if he ever does this*; but that in the former case it is essentially equivalent to *εἰ τοῦτο πράξει* (at least in respect to time), and would be regularly expressed in Latin by *si hoc faciet*; whereas in the latter it has no resemblance in meaning to *εἰ τοῦτο πράξει* or *si hoc faciet*, but would be commonly expressed in Latin by *si hoc facit*, occasionally even in Greek taking the form *εἰ τοῦτο πράσσει*.

The idea of "possibility" or something of the kind being attached to the subjunctive, it was naturally supposed that the simple indicative\* in protasis must have a corresponding idea at its foundation, and that of "certainty" or "reality" has

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\* The expression "simple indicative" is meant to include all indicatives in protasis except the past tenses implying non-fulfillment of the condition.

generally been assigned to it. Some of the best grammarians (as Krüger) have avoided this rock, and have seen that no such idea is implied when we use the indicative in protasis, which can express the condition *if all men are liars* as well as the condition *if truth is eternal*. But this gain is more than balanced by the increased vagueness in the idea of "possibility," which now stands attached to the subjunctive with no corresponding "idea" in the indicative to contrast it with. If every condition that is "objectively possible," or "possible now or in the future," or "possible with a prospect of decision," or "uncertain with a prospect of decision," requires the subjunctive,—and what else is to be inferred from the rules?—in what cases is a pupil to be taught to use the indicative? How is he to know that he *must* use the indicative, and not the subjunctive, to express, for example, *if the enemy already knows our plans (which time will show), he is well prepared?* To take an actual case—one which time has somewhat illustrated—in a review of Farrar's Greek Syntax, in the North American Review in 1868, I said that the condition *if Livingstone is now living* (or, if that is preferred, *be now living*) could be expressed in Greek or Latin only by the present indicative; and yet this was then a most striking example of "possibility (or "uncertainty") with prospect of decision." Now was there any view of the possibility or impossibility, certainty or uncertainty of Livingstone's safety at that time, which would have justified any one in using the subjunctive to express this condition? Would the indicative be any more correct, or the subjunctive any less a blunder, now (July, 1873) than when almost every one despaired of the great traveller's safety? And yet what schoolboy, if he had followed the common rules in his grammar, would have used anything but the subjunctive? If now any grammarian has subtlety enough to explain away this difficulty (which is fairly stated as it once actually occurred in my own experience), it must be done by refining "possibility" to an abstraction which will be entirely beyond the reach of schoolboys, and utterly ridiculous as an explanation of one of the most common forms of Greek syntax.

Probably no grammarian would now maintain the absurdity that the indicative in protasis expresses either *certainty in fact* or *what is believed by the speaker to be certain*. Here, however, has always been a fine field for grammatical logic. Few have the courage to take the bull by the horns as Jelf does, when he tells us (*Grammar*, § 853) that a speaker or writer sometimes suppresses his real opinion for politeness (!) or for argument's sake, adding that it will generally be found that the protasis and apodosis *taken together* express his true opinion,—apparently forgetting that when “the protasis and apodosis taken together,” i. e. the whole statement, does not express the speaker's real opinion, either in Greek or in English, it is a case of lying, not of false syntax! Most grammarians are eager to disclaim any connection between the “certainty” here intended and matter of fact or even of opinion; and they thus reduce the “certainty” to a harmless abstraction, which is utterly valueless as a definition. Thus Zumpt (*Latin Gram.* § 517, note) explains, with regard to the Latin indicative in protasis, that what is *assumed* as certain with respect to the inference (*in Bezug auf die Folgerung*) need not *be* certain either in fact or in the speaker's belief. In all this I can see nothing more than is necessarily involved in the very idea of a “supposition:” we *suppose* or *assume* something as happening, or some state of things as existing, in the past, the present, or the future; and we then state a result or conclusion which followed, follows, will follow, or would follow from the realization of the supposition or assumption. If this is all that is meant, it is hard to see why even suppositions implying non-fulfillment of the condition need be excluded from those which “assume something as certain (or “real”) with respect to the inference:” when we say *if Philip had died, we should have remained free*, we suppose or assume something to have happened (although we imply that it did not happen) with a view to a result or conclusion which we are about to state. What is there in the least more absurd in this than in applying Zumpt's principle to *εἰ ἦγώ Φαῖδρον ἀγνοῶ, καὶ ἐμαντοῦ ἐπιλέλησμαι· ἀλλὰ γὰρ οὐδέτερά εστι τούτων* (*PLAT. Phaedr. 228 A*)? Zumpt would include

examples like *si naturam sequemur ducem, nunquam aberrabimus*, under the same principle of “certainty” with *si vales, bene est*; but a Greek writer would naturally express the former protasis by the subjunctive, the latter by the present indicative.

The idea of “certainty” then must be deprived of its most characteristic attributes before it can apply to every present indicative in protasis; the same process is necessary before the idea of “possibility” can apply to every subjunctive in protasis. It is perhaps true, in one sense, that when we “suppose” a future event we assume its possibility; for how otherwise can we consistently suppose it to take place? For example, if we translate into Greek *if the sky falls, we shall catch larks*, we must use *ἴαν* and the subjunctive; and it might edify some teachers to hear a docile pupil explain such a subjunctive as used to express “possibility with a prospect of decision.” So far at least must the meaning of “possible” be extended: it thus becomes equivalent to “susposable.”

What then is the distinction between the subjunctive and the indicative in protasis? I have not criticised the common distinction with a view to proposing another of a similar nature, but to justify myself in maintaining the position which I assumed with great hesitation thirteen years ago, that no distinction of this character was ever present to the mind of a Greek. I have nothing now to change in the statement which I made in 1864,\* although I am aware that it has been looked upon by many whose names I most highly respect as containing “dangerous heresies.” “In one point all these authorities agree,—in looking for some principle on which the use of the subjunctive depends, to be found either in the nature of the act supposed or in the manner in which the speaker conceives it. This, it seems to me, is the rock on which they have all split. After the most careful study that I have been able to give to the subject, and especially after a comparison of several thousand classic examples, I am convinced that no such principle can be found. Every example that I have met with has only confirmed the opinion, which I can now express

\* See Proceedings of the American Academy of Arts and Sciences for Dec. 6, 1864: vol. vi., p. 367.

with the greatest confidence, that there is no inherent distinction between the present indicative and the present subjunctive in protasis (between *εἰ βούλεται* and *ἴαν βούληται*) except that of *time*."

So far as the indicative is concerned, the inherent distinction of time is perfectly obvious; and if we had no other mood to consider, it would be plain that in both Greek and Latin we can express a supposition in any time by simply using the proper tense of the indicative. Thus we can express by the indicative *if he is doing*,—*if he did*,—*if he has done*,—*if he had done*,—*if he shall do*,—*if he shall have done*,—with no implied assumption of truth, certainty, uncertainty, possibility, or probability, and with no other distinctions than those which belong to the same tenses in any other kind of sentence. The trouble begins when we attempt to define the use of the Greek subjunctive. Here the whole difficulty—indeed, the whole supposed necessity for any definition at all except that of time—seems to me to arise from confounding two distinct uses of the subjunctive in protasis. In one of these the subjunctive always refers to future time, and hardly differs from the future indicative; in the other it expresses a shade of meaning which (so far as I know) no other language has ever undertaken to distinguish in its ordinary usage from that of the present indicative. Each of these has a use of the optative corresponding to it; and these optatives have generally been carefully distinguished by grammarians. No one now thinks of bringing under the same head (at least for practical use) such instances as *εἰ τίνας θορυβουμένον αἰσθούσθω*, *κατασβενύναι τὴν ταραχὴν ἐπειράτῳ* (XEN. Cyr. v. 3, 55) and *εἰ τίνας αἰσθούσθω, πειρῶτο ἄν*, etc., the difference in time here making that in construction too obvious to be mistaken. And yet there is just as important a difference between *ἴαν τίνας αἰσθηται, πειρᾶται*, *if he ever sees any, he (always) tries*, etc., and *ἴαν τίνας αἰσθηται, πειράσται*, *if he shall see any, he will try*, etc.; although here the slighter distinction in time has caused that in the construction to be overlooked. The neglect of this distinction, and the consequent attempt to unite both uses of the subjunctive under one "idea," have caused the whole

difficulty. Where the cases to be accounted for were so dissimilar, the theory had to be loose and accommodating. If the Greek used its subjunctive in conditions of the latter class alone, it seems impossible that any one could overlook the simple truth, that *εἰ πράσσει τοῦτο* means *if he is doing this (now)*, and *ἴαν πράσσῃ τοῦτο* means *if he shall do this*, the latter not differing, except in vividness of expression, from *εἰ πράξει τοῦτο*. In the other class (as *ἴαν τίνει αἰσθηται, πειρᾶται*) the subjunctive clearly does not refer to the future, as is obvious if we try to substitute *εἰ αἰσθησεται* in the protasis; neither does it refer to the present exactly, for there is a great difference between *if he is now perceiving* and *if he ever perceives*. This is a distinction which perhaps no language but the Greek ever expressed systematically by its construction, other languages generally contenting themselves by using the present indicative where the Greek uses this subjunctive, as they use the past tenses of the indicative for the corresponding optative. Such conditions, as they do not refer to a definite act or even to a definite series of acts, but *indefinitely* to *any one* of a series or class of acts, may be called "general conditions," *if* here having the force of *if ever or whenever*.

I cannot claim to have first called attention to the existence of these general conditions. As I have said, those referring to the past have been generally recognized; and at least one writer (Bäumlein) states that the Greek subjunctive in protasis is sometimes general in its nature. As I have been misunderstood on this point, I cannot state too distinctly here, that the chief peculiarity of the classification here proposed consists in showing (first) the close relation between the optative and the subjunctive in general conditions, and (secondly) the important result of this connection, viz. that, as the past general conditions expressed by the optative are a "variation" (so to speak) of the ordinary past conditions expressed by the indicative, so the quasi-present general conditions expressed by the subjunctive are a variation of the ordinary present conditions, which other languages (and sometimes even the Greek) express by the present indicative. On the other hand, Bäumlein leaves the subjunctive in general conditions, as well

as in other kinds of protasis, to be explained on his single principle as denoting a “Tendenz zur Wirklichkeit;” and he seems to have no suspicion that the two subjunctives stand in different relations to the present indicative. His remark therefore shows a sense of the weakness of his general theory, but does not help us to an understanding of the relation of the subjunctive to the indicative. A still more remarkable case of hesitation in stating a general definition of the force of the subjunctive is found in the new edition of Kühner’s larger Grammar (§ 394), where he says that the subjunctive properly refers to future time, although sometimes in dependent clauses *it seems* to refer to present time, really, however, expressing only what is “assumed as present.” It is almost needless to say, that the examples of this singular exception are found in the general conditional sentences above-mentioned. We must confess that, with all its looseness, Kühner’s remark comes nearer a true definition of the subjunctive than any which omit the element of time altogether.

The distinction of general and particular suppositions is not confined to the two cases which have been discussed. It extends to all conditions in present, past, and future time; but as it effects the construction only in the cases in question, the others may be neglected in a classification which belongs to syntax alone. We need therefore set apart as a special class only the present and past general conditions above described, which the Greek so peculiarly expresses by the subjunctive and optative instead of merging them (as other languages generally do) with other present and past conditions which take the simple indicative. It will be borne in mind then that all the classes of “ordinary conditions” in the classification which follows, except the first, contain both particular and general suppositions, and even in the first the distinction is sometimes (though rarely) neglected.\* It seems to me that,

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\* I must here acknowledge and correct a former error. In the first edition of “Greek Moods and Tenses” (1860), the distinction of particular and general suppositions was (in the classification) carried into future conditions, although no distinction in construction is there made; subsequently (1865) this arrangement was given up as cumbrous, and it was stated in a note that general future conditions were included under the rules for particular future conditions. I am under

when attention has once been called to the true position of the subjunctive in present general conditions, it will need no further argument to show that its essential character in all other cases of protasis is its designation of *future time*; so that *ἴαν τοῦτο γένηται* here must mean *if this shall happen*, and the subject may be *the sky falling*, or *to-morrow's sun rising*, or any other supposable event, whether possible or impossible. Any further definition would here seem superfluous.

The statement that *ἴαν τοῦτο γένηται*, apart from present general conditions, always means *if this shall happen*, may strike some with surprise, especially such as have been in the habit of drilling pupils in certain well-known "pattern" sentences, ingeniously written by modern grammarians to illustrate rules which (like the examples) are purely of their own invention. Thus *ἴαν τι ἔχω, δώσω* cannot possibly mean (as it seems expected to) *if I (now) have anything (which will hereafter be proved), I shall give it*; it is Greek only in the sense *if I shall (hereafter) have anything, I shall give it*. So *ἴαν τοῦτο λέγεις, ἀμαρτάνεις* can mean only *if you ever say this (i. e. so often as you say this) you err*; which is not at all what boys are expected to understand by it when they see it tortured into bad Latin, *si hoc dicas, erras*, and into unintelligible English, *if you say this, you err*, all the time innocently imagining it was written by some Greek and will teach them to imitate Demosthenes and Plato! If the discussion is confined to the writings of classic authors, no examples of *ἴαν* with the subjunctive will be found which do not belong clearly to one or the other of the two classes above explained; and every scholar should enter an earnest protest against the common practice of instructing pupils by means of sentences which have been made to suit modern theories, and which

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great obligations to Dr. B. L. Gildersleeve, Professor in the University of Virginia, for valuable criticisms on this arrangement; from these, and from a note in the latest edition of his Latin Grammar (under Conditional Sentences), it is evident that there is no logical propriety in excluding the distinction in question even from the class (I. a, 2) of present and past conditions implying non-fulfillment. It has therefore seemed better to make a special class (II.) of present and past general conditions, to be treated as a variation of class (I. a, 1), and then to treat all other conditions (both particular and general) under the head of "Four Forms of Ordinary Conditional Sentences."

conceal from view or violate the real principles involved in classic constructions. I do not quote classic examples here in support of my own theory, partly because I have done this elsewhere, and partly because I wish to ask any one who needs such confirmation to simply turn to any piece of classic Greek which contains conditional sentences and test the question by examples of his own choice.

The relation of the subjunctive to the optative in protasis remains to be considered. The most common doctrine is that the subjunctive implies "possibility with prospect of decision," the optative "possibility without prospect of decision." There is also a general opinion that the optative implies less probability or more uncertainty than the subjunctive. I confess, this question is by no means as simple as the former; and in first proposing the classification here advocated, in 1860, I accepted the former of these distinctions in a modified form. But later consideration has made me more and more doubtful whether any such distinction ever occurred to the mind of a Greek. When the optative in past general suppositions is excluded, it is evident that the optative in ordinary protasis refers to the future. This important character of this optative is seldom made prominent by grammarians. But how does this futurity differ from that expressed by the subjunctive? Fortunately, we have the same distinction in English; for I cannot think any one will seriously doubt that, whatever difference was felt in Greek between *ἴαν τοῦτο γένηται* and *εἰ τοῦτο γένοιτο*, in the cases in question, is still felt in English between *if this shall happen* (or *if this happens*, in a future sense,) and *if this should happen*. If this is granted, we may simply say that, wherever we should use the latter form in English, the optative would be used in Greek; and wherever we should use the former, the subjunctive or future indicative would be the natural Greek form. Now if any one is distinctly conscious of always implying greater "probability" or more "prospect of decision" when he says *if this happens* (or *shall happen*) than when he says *if this should happen*, it will be hard to convince him that the Greek did not make the same distinction; and it may be that he has unconsciously

conformed his own usage in English to what he assumes to be the correct usage in Greek. I doubt exceedingly whether any one who never studied Greek (if such a person could be a competent judge of modal forms in any language) would explain the English usage in this way; indeed, it is one of the hardest things in the world to state exactly the distinction which is felt between two such similar forms,—for which reason it is extremely easy to imagine it to be one of those in question or almost any other impalpable distinction that may be suggested. Every one will admit that it is often indifferent which of the two forms is used; and in such cases it is sometimes hard to attach the formula required by the common rules to each form of protasis. Does the proverb “If the sky falls, we shall catch larks” imply any greater “prospect of decision” than it would in the form “If the sky should fall, we should catch larks”? Did Demosthenes (*Phil.* i. p. 43, § 11) imply that there was any nearer prospect of decision on the question of Philip’s death when he referred to it in the words *ἄν οὖτός τι πάθῃ*, than when he repeated his supposition in the very next sentence in the form *εἴ τι πάθοι*? Is not the essential distinction here merely one of vividness of expression or distinctness in the form of the supposition, entirely apart from any difference of the speaker’s opinion on any subject? If this is admitted for English, the burden of proof surely rests on him who maintains (as too many do, at least in practice) that the ancients had entirely different modes of thought from ourselves, and that what seems plain common sense in English may involve metaphysical subtleties in Greek. If this view is correct, the optative in ordinary protasis is merely a vaguer or less vivid form than the subjunctive for stating a future supposition, bearing a relation to the subjunctive somewhat similar to that which the subjunctive itself bears to the future indicative. Thus we have three forms which may be used to express a future condition, differing essentially only in the vividness with which they state the supposition,—*εἴ γενήσεται*, if it shall happen; *εὰν γένηται*, if it happens (i. e. shall happen); and *εἴ γένοτο*, if it should happen.

I am far from denying that, when the subjunctive and

optative are brought into contrast in successive sentences, the subjunctive may be used in the supposition which the speaker regards as the more probable, the more likely to be fulfilled, the more dangerous, or which is for any other reason the more prominent in his mind. These distinctions, however, seem to me to stand to the more comprehensive one of greater and less vividness in the relation (if I may be allowed the expression) of species to a genus. If a speaker has at his command two forms for expressing substantially the same kind of supposition, one of which is more vivid than the other, he will naturally choose the former for a supposition which he wishes to contrast with another in any of the respects above mentioned. For this reason the future indicative may be used to express a more prominent supposition more vividly, and the subjunctive to express a less prominent one less vividly. Neither the future indicative nor the subjunctive nor the optative expresses any *absolute* amount of vividness or probability; it is only by contrast that these qualities sometimes appear *relatively*. In DEM. Cor. pp. 286, 287, § 176, we find *εἰ προαιρησόμεθα* in a supposition which the orator wishes to make especially vivid that he may warn his hearers against the consequences of the fulfillment of the condition; still, it is a condition which he hopes and prays may never be fulfilled, and which in fact never was fulfilled. In the next sentence he uses *ἄν πεισθῆται εἶμοι* to express what he hopes will happen and what does actually happen; but as he reserves the substance of his plan for the next sentence, the weaker form here would seem to give greater prominence to the warning of the previous clause. This, however, is dangerous speculation; for there can be little doubt that the two forms were sometimes used when it is next to impossible that any deliberate plan could have affected the choice. Thus, when Isocrates (Archid. p. 138 A.) says, *ἢν ἐθέλωμεν ἀποθνήσκειν ὑπὲρ τῶν δικαίων, εὐδοκιμήσομεν εἰ δὲ φοβησόμεθα τοὺς κινδύνους, εἰς πολλὰς ταραχὰς καταστήσουμεν ἡμᾶς αὐτούς*, it seems absurd to refine on the possible reasons for using the moods as he does rather than in the inverse order. All we can say with certainty is, that the Greek language had this variety of forms, which *could* be used

to express nice distinctions of thought, just as it had the distinction of the present and aorist subjunctive and others of the same nature ; but it by no means follows that the Greeks *always* used their finest tools.

This not uncommon use of the future indicative and the subjunctive in successive conditions serves to illustrate the much rarer use of the subjunctive and optative in antithesis. The two favorite examples of the latter are in DEM. Cor. p. 276, §§ 147, 148 ; where it is said that the two optatives *εἰ συμπειθοί* and *εἰ εἰσηγοίτο* introduce “the more improbable alternative” and “the condition which did not happen,” as opposed to *ἴαν αἱρεθῆ* and *ἄν ἦ*, which state “the very condition which was actually fulfilled,” etc. (Holmes.) All this is very true. But it will be noticed that the two optatives belong to conditional sentences depending on past tenses in *oratio obliqua*, and *for this reason alone* are in the optative ; in the direct form in the speaker’s mind all four conditions would have the subjunctive, and after a present or future tense the subjunctive *must* have been retained in all alike. After a past tense, where the option between the original forms and the same tenses of the optative was allowed, the orator twice chooses the more vivid form, that in which the idea was originally conceived, to express what (he implies) Philip had most at heart, and the optative to express the opposite alternative. I cannot believe that there would have been any difference in the use of the moods here if Philip’s plan had failed, for there would still have been the same ground for distinguishing the two sets of conditions in respect to vividness. This example suggests and illustrates the remark to which the preceding argument has been tending, that the subjunctive and optative in common protasis may be said to differ very much (if not precisely) as they do in the dependent clauses of *oratio obliqua* after past tenses. The comments which I have quoted on the passage of Demosthenes show a feeling that this is so. It will be generally admitted that the direct form *ἴαν ἐλθω, τοῦτο ποιήσω* can be expressed indirectly either by *ἔφη ἴαν ἐλθη τοῦτο ποιήσειν* or by *ἔφη εἰ ἐλθοι τοῦτο ποιήσειν*, with no essential difference of meaning, the former being the less

common but more vivid form. What now could be more natural than that *εἰν* *ελθη* and *εἰ ελθοι* should differ here very much as they would differ in any other kind of sentence? The same principle, I believe most firmly, holds in all similar cases in which option is allowed between the subjunctive and optative or between the indicative and optative, in constructions which partake of the nature of *oratio obliqua*. No other principle accounts satisfactorily for the frequent use of the subjunctive in final clauses after past tenses in the same sense as the optative, a usage familiar to all readers of Thucydides, and often explained in the most wonderful manner by commentators on particular passages. No one can try to apply the common rule that the subjunctive "brings the action of its verb down to the present time" to ten passages in succession without seeing its utter absurdity. As all final clauses express the thought of the one who conceived the purpose, they are so far affected by the principles of *oratio obliqua* that they allow, after past tenses, either the original subjunctive or the same tense of the optative, the former being the rarer but more vivid form of expression.

All forms of conditions have now been considered except those of present and past time which imply non-fulfillment. These are too familiar to need comment: one caution, however, is sometimes necessary against our old enemy "possibility." This construction implies merely that the condition *is not* or *was not fulfilled*; the supposition of the protasis, however, may be a *possible* one or an *impossible* one, according to circumstances. There is no more impossibility implied when we say *if twice three were seven* than when we say *if twice three are seven*, unless no more is meant by impossibility than is already involved in the non-fulfillment of the condition,—in which case the addition is superfluous. This confusion is especially to be avoided in defining the forms of wishes, which are conditional sentences without the apodosis. Wishes are often divided into "possible," expressed by the optative, and "impossible," expressed by the indicative: as if the wish *O that our friends were here!* were "impossible" except from the present being beyond the chance of change, or as if *O that*

*the heavens would fall* were “possible” except from the future being open to unlimited possibilities. It is clear that here, as in protasis, time is an essential matter in both classes, and possibility need not be considered.

To sum up the results of this discussion in a tabular form, we have

I. Four forms of Ordinary Conditional Sentences, two with present and past, two with future conditions.

(a.) 1. Present and past (particular) conditions implying nothing as to fulfillment. Indicative with *ei* in protasis: any verbal form in apodosis. *Ei πράσσει τοῦτο, καλῶς ἔχει, if he is doing this, it is well.*

2. Present and past conditions implying non-fulfillment. Past tenses of indicative with *ei* in protasis: same with *āv* in apodosis. *Ei ἐπράσσε (ἐπράξε) τοῦτο, καλῶς ἀν εἶχεν (ἔσχεν), if he were doing (had done) this, it would be (would have been) well.*

(b.) 1. Future conditions (more vivid form). Subjunctive with *éav* (sometimes future indicative with *ei*) in protasis: future indicative or some other future form in apodosis. *'Eav πράσσῃ τοῦτο (εἰ πράξει τοῦτο), καλῶς ἔξει, if he shall do this, it will be well.*

2. Future conditions (less vivid form.) Optative with *ei* in protasis: optative with *āv* in apodosis. *Ei πράσσοι τοῦτο, καλῶς ἀν ἔχοι, if he should do this, it would be well.*

II. Two forms of General Conditional Sentences, one present and one past.—the apodosis expressing a customary or repeated action or a general truth.

(a.) Present general conditions after verbs of present time: subjunctive with *éav* in protasis. *'Eav τις τοῦτο πράσσῃ, καλῶς ἔχει, if any one (ever) does this, it is (always) well.*

(b.) Past general conditions after verbs of past time: optative with *ei* in protasis. *Ei τις τοῦτο πράσσοι, καλῶς εἶχεν, if any one (ever) did this, it was (always) well.*

N. B. The last two forms are variations of I. (a.) 1, and are the only forms of general conditions which are distinguished by the construction. All others, therefore, are included in the last three forms of ordinary conditional sentences, (a.) 2, and (b.) 1 and 2.

The discussion in this paper has been confined to general principles, and all such matters as the use of *εἰ* for *ἴαν* with the subjunctive, the omission of *ἄν* in the apodosis, as well as all the combinations of one form of protasis with another form of apodosis, have been excluded. It is important, however, to notice the exact correspondence between the forms of protasis and those of conditional relative sentences, which becomes clear, as it seems to me, only when the present classification is adopted. It will be sufficient to give examples under the proper numbers.

I. (a.) 1. "Ο τι ἔχει, δώσει, *he will give whatever he (now) has.*  
Α μὴ οἶδα, οὐδέ σοιμαι εἰδέναι. PLAT. Apol. 21 d.

2. "Ο τι ἔσχεν, ἔδωκεν ἄν, *he would have given anything that he had* (implying that he had nothing, like *εἴ τι ἔσχεν*). Οὐκ ἀνέπειροῦμεν πράττειν ἀ μὴ ἡπιστάμεθα. PLAT. Charm. 171 E.

(b.) 1. "Ο τι ἄν ἔχῃ, δώσει, *he will give whatever he has* (i. e. shall have). "Οταν δὲ μὴ σθένω, πεπαύσομαι, *when I shall have no strength, etc.* SOPH. Antig. 91.

2. "Ο τι ἔχοι, δοιη ἄν, *he would give whatever he might have* (commonly, whatever he had). Φάγοι ἄν ὅπότε βούλοιτο, *he would eat whenever he pleased.* XEN. Mem. ii. 1. 18.

II. (a.) "Ο τι ἄν ἔχῃ, δίδωσι, *he (always) gives whatever he has.\** Συμμαχεῖν τούτοις ἐθέλουσιν ἄπαντες, οὓς ἄν ὤρῶσι παρεσκευασμένους. DEM. Phil. i. p. 42, § 6.

(b.) "Ο τι ἔχοι, ἔδιδον, *he (always) gave whatever he had.* Οὐς ἴδοι εἰπάκτως ιόντας, τίνες τε εἰεν ἥρώτα, καὶ ἐπεὶ πύθοιτο ἐπήνει. XEN. Cyr. v. 3. 55.

In conclusion, I add a few remarks on the English subjunctive in protasis, although I am well aware of my inability to deal properly with this subject. The English of our time, especially the spoken language, generally makes no distinction between present and future time in protasis, using *if he does this* to express all the various meanings which the Greek

\* Here we sometimes find the indicative, especially with *ὅστις*, that pronoun expressing the indefiniteness of the general condition sufficiently without the help of the verb. See SOPH. Antig. 178 (*ὅστις μὴ ἀπέραι*), and compare Odys. xiv. 157 with II. ix. 313. So sometimes in the past form. This neglect to mark the general condition by the form of the verb occurs sometimes in common conditional sentences. See SOPH. Trach. 944.

expresses by *εἰ τοῦτο πράσσει* (in one sense), *εἰ πράξει*, and *ἴαν πράσσῃ* (or *πράξῃ*), and the Latin by *si facit* and *si faciet* (or *fecerit*). Some of our American Solons, among others those of the Commonwealth of Massachusetts, have recently abolished the subjunctive as well as the future indicative in protasis, so far as they can, by expunging both from the statute books; so that some of our laws have "Whoever steals," "If a clerk embezzles," etc., instead of the time-honored forms, "Whoever shall steal," "If a clerk shall embezzle" or "If a clerk embezzle." In the Massachusetts riot-act we now find, "If any persons.....*are* unlawfully, riotously, or tumultuously assembled in any city or town," etc. In Athens at least a law thus expressed would have been worthless against any rioters who were not already assembled when the law was passed. Still there is no doubt that this is the common English form, authorized by modern usage; although it is to be regretted that our language should lose its power of expressing nice distinctions of thought,—a power which especially distinguishes the ancient languages, and the Greek preëminently, from the modern. For example, the English sentence, *he said that, if they should pass this vote, the State would be saved*, could be expressed in Greek in sixteen or more distinct forms, each depending on some delicate shade of meaning, or some degree of vividness or emphasis, which no modern language would attempt to express, the changes being confined to the last two verbs. It is one mark of the degeneracy of the modern Greek that it has lost the ancient distinction between the subjunctive and the indicative; *γράψῃ* and *γράφῃ*, not being distinguished in pronunciation, have now lost their distinctive force to the mass of the people. The scholars in Greece are doing their best to revive this, as well as other distinctions of the ancient language of their country, by observing the proper spelling in the written language: it would seem as if our tendency were rather to abolish whatever distinction of the kind has been left to us, and to make our present indicative do the work of both present and future.

Still the English has a subjunctive, which is distinguished from the indicative in most verbs only in the third person

singular ; and it is still in good use, although it is to be feared that the levelling power of custom will soon obliterate it entirely. But on what principle do modern writers use the English subjunctive after *if*? I think that many writers would admit that they use it without thinking of any special distinction between *if it be* and *if it is*; while others are influenced by the supposed distinction between the corresponding classic forms, *if it be* being used where doubt is to be expressed, *if it is* where the writer believes his supposition is correct. How far such a distinction in English is now authorized by usage I will not pretend to say; it has been one of the chief objects of this paper to show that no such distinction is found in either Greek or Latin.

If we look at the English translation of the Bible, which represents the language when the subjunctive was in full use, we find the Greek subjunctive in the New Testament invariably translated by the subjunctive or the future (except where it is expressed by a participle), never by the present indicative. But this investigation proves too much; for the same translation is equally consistent in expressing the Greek present indicative by the English subjunctive. Thus “if it fall,”—“if any man shall say unto you,”—“if a house be divided,”—“if any man say unto you,”—“if thy brother trespass against thee, rebuke him, and if he repent, forgive him,”—“if another shall come,”—are translated from *ἐὰν ἐμπέσῃ*,—*ἐάν τις εἴπῃ*,—*ἐὰν οἰκία μερισθῇ*,—*ἐάν τις εἴπῃ*,—*ἐὰν ἀμάρτῃ ὁ ἀδελφός σου*, . . . . καὶ *ἐὰν μετανοήσῃ*,—*ἐάν ἄλλος ἔλθῃ*. But we also find, “if the light that is in thee be darkness,” *εἰ τὸ φῶς σκότος ἐστίν*,—“if Satan cast out Satan,” *εἰ ἐκβάλλει*,—“if he be Christ,” *εἰ οὐτός ἐστιν ὁ Χριστός*,—“if David call him Lord,” *εἰ καλεῖ*,—“if any man have not the spirit of Christ,” *εἴ τις πνεῦμα οὐκ ἔχει*. It is plain that no principle as to the distinction of the subjunctive and indicative can be derived from this source; and yet here, if anywhere, the Greek distinction would have been followed if it had been recognized. But although no such forms as “if he does” or “if it is” are found in our Bible, it will be noticed that the form in *-est* and similar forms of the second person singular were allowed after *if*, as if these forms were

looked upon as belonging to the subjunctive also. Thus (Matth. v. 23), "If thou *bring* thy gift to the altar, and there *rememberest*," etc. Compare (Exod. xx. 25) "And if thou *wilt* make me an altar, . . . for if thou *lift* up thy tool," etc. The form in -eth is very common in conditional relative sentences, where the future indicative is regularly used; thus, "whosoever *toucheth*,"—"whosoever *looketh*;"—but also, "whosoever *shall put away* his wife,"—"whosoever *shall marry*,"—"whoso *sheddeth* man's blood."\* We find as little help in the language of Shakespeare; thus we have in Macbeth, "If such a one be fit to govern, speak;" "If it be mine, keep it not from me;" "Let me endure your wrath if't be not so;" but just below the last example, "If this, which he avouches, does appear." In Bacon (Maxims of the Law, ix.) we find the following: "If I. S. *devise* land by the statute of 32 H. VIII., and the heir of the devisor *enters* and *makes* a feoffment in fee, and *feoffee dieth* seized, this descent bindeth." So, "If the land after *descend* to me, I shall never be remitted." Again (Ibid. xii.), "If a man *recovers* by erroneous judgment, and *hath* issue two daughters, and one of them *is attainted*, the writ of error shall be brought," etc. In turning over the pages of the Spectator, I find fifty instances of the present indicative after *if*, without meeting any of the subjunctive; this can hardly be accidental. The following extract from Macaulay's Essay on *Church and State* will not disclose very plainly the principle which that writer followed: "If the propagation of religious truth *be* a principal end of government, as government; if it *be* the duty of a government to employ for that end its constitutional power; if the constitutional power of governments *extends*, as it *most unquestionably does*, to the making of laws for the burning of

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\* In the Lord Chief Justice's charge in the Tichborne trial (170th day) are some excellent remarks on the accuracy of the French in saying "when he shall come," etc., where in English we say "when he comes," etc. The defect here noticed, however, is seen chiefly in modern English; for the English of the Bible is as accurate as French, and in some respects even more so. Thus in 2 Cor. iii. 16, we have *when it shall turn=quand il se tournera*; see also 1 Cor. xv. 28; Luke xii. 10, *whosoever shall speak=quiconque parlera*; see also John xv. 16. But in John v. 43 (above quoted in Greek) we have *if another shall come=si un autre vient*, where the old English had the advantage; see also Luke xii. 38, xix. 31.

heretics ; if burning *be*, as it most assuredly is, a most effectual mode of suppressing opinions, why should we not burn ? If the relation in which government ought to stand to the people *be*, as Mr. Gladstone tells us, a paternal relation, we are irresistibly led to the conclusion that persecution is justifiable." Just below he says : " If a boy *plays* truant at church-time, a task is set him." Again, we find *if it be true* and *if experience shows*, both in the same paragraph. It might be thought that Macaulay was unwilling to use any other subjunctive than *be*, which indeed is the one that most frequently occurs in modern English. Mr. Gladstone writes (*Studies on Homer*, i., p. 18), "If Homer *is* not fully studied in our universities," and in the next page, "If my estimate of those purposes *be* correct." Again (p. 39), "If it *be* contended," and (p. 80) "If such there *has* been."

These instances are quoted here not by way of criticism, but partly to show the utter want of any principle in modern English on the subject, and partly to incite some one who can speak with authority on English syntax to investigate the question historically, and show us, if possible, what is the correct usage according to the traditions of the language. If it is true (or if it *be* true), as I fear it is, that no one can define the correct usage of the present day, even so far as to tell us what is the distinction recognized by our best writers between *if it be* and *if it is*, or if no two opinions on this question would agree, such uncertainty and such laxity of usage are surely no credit to our scholarship or to our language.

VII.—*Recent Discussions of Grimm's Law.*

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THE Early English Text Society have lately given us an edition of Alfred's translation of Gregory's *Pastorale*, carefully printed from manuscripts of the age of Alfred. It is a book of great value for many reasons. The text makes it possible now to study with confidence the language of Alfred in the minutest points. The introduction is also of value, and the translation, and the notes. They point out with a good deal of care the characteristics of Alfred's English. The editor, Mr. Henry Sweet, is not only an accomplished linguist, and palæographer, but also an earnest phonologist. He began his phonetic career with the study of Mr. Bell's *Visible Speech*, and is in full sympathy with Mr. Bell and Mr. Ellis in their purpose to lay the foundations of the Science of Language in the scientific study of living speech from the physiological side. He has spent a summer studying the Danish pronunciation in the mouths of living Danes, and written a paper on it for the Philological Society, which Mr. Ellis speaks of as "one of the acutest phonological investigations of recent times."

In studying the early manuscripts of Anglo-Saxon, he has been greatly interested in the changes in the spelling, and thinks much light is thrown on the history of speech by his observations. He has studied with special care the history of the lingual spirants, or aspirates, commonly represented in our phonographic systems by *th* and *dh*, and he adds to his edition of the *Pastorale* an essay on this subject, which is also an essay on Grimm's Law. It is a familiar fact that there are two signs in Anglo-Saxon manuscripts for these spirants, one a rune (*p*) called *thorn*, and the other (*d*) a crossed *d*; and it had been supposed that the first (*p*) originally represented the surd (*th*) heard in *thin*, *path*, and the second (*d*)

the sonant (*dh*) as heard in *thine, other, smooth*. It was well known that none of the manuscripts had been found to carry out this distinction with any thoroughness, but a prevailing tendency to use *p* at the beginning of words and *d̄* in the middle and at the end of words, which is found in the best known manuscripts, was thought to indicate a general habit of the Anglo-Saxon to use the surd sound at the beginning and the sonant in the middle and at the end of words, as is the habit in modern Icelandic. Mr. Sweet's more careful examination of the oldest manuscripts leads him to state that, in each of these, one mode of writing this spirant is used throughout. The very earliest use the Roman letters *th* only. Then *p* and *d̄* appear, and some manuscripts have one and some the other. Those of the *Pastorale* have *d̄* regularly, initial, medial, and final. The Parker manuscript of the Chronicle, which is of equal antiquity, used only *p*. Isolated examples occur, however, which show that both scribes knew both signs. Mr. Sweet believes that when these manuscripts were written which use but one sign, only one sound existed, and this, he thinks, was the sonant (*dh*). The English surd (*th*), he says, is everywhere a later weakening of the sonant.

It is well known, however, that this letter (*p, d̄*) corresponds to the surd mute *t* of the Sanskrit, Greek, and Latin, and it is difficult to believe that *t* would change to *dh*. Mr. Sweet admits the difficulty, and meets it by the hypothesis that *t* first changed to *d*, then *d* to *dh*. Both these changes are regular weakenings, and thus far the hypothesis looks fair. But they are inseparably connected with many other changes. What is called Grimm's Law is a formula for a great number of undeniable facts, closely related to each other, and constituting the systems of mutes and their spirants in all the Teutonic tongues. The formula is, "surd mutes change to their aspirates, sonants to their surds, aspirates to their sonants." If the formula be applied to a word in the Parent speech and generally to a word in Sanskrit, Greek, Latin, and the like, it will give the right letters for the corresponding word in Gothic, Anglo-Saxon, or other Low German speech. If it be applied to a word in a Low German speech, it will give the letters for the corresponding word in High German.

It will be noticed in the first application of the law that it is really the Parent Speech which changed to Low German, not Latin, or Greek, or Sanskrit. These are generally like each other, and like the Parent Speech, but not always. In Latin especially there have been great changes among the aspirates. The changes of Grimm's Law are widely anticipated, especially between vowels. But besides this, *th* and *dh* have often, perhaps oftenest, changed to *f*, or with a secondary change, to *b*. We must not try to change *for-is* to *door*, or *ruf-us* or *rub-er* to *red*, but remember θύρα and ἵ-pυθ-πός, and, if we can, the Sanskrit *rōdh-i-tas*, and replace the original lingual aspirate.

It should be noticed also that words borrowed from other tongues by the Germans are not regularly changed, and that onomatope produces similar words in different tongues not historically connected, and prevents words from changing, as κόκκοξ, Lat. *cuculus*, Ger. *kuckuck*, cuckoo. These mutes occur also in combinations in which the letter to which they would change is hard to pronounce, and then they do not change, or at least according to the letter of the law. Such are the combinations *nd*, *ld*, *st*, *sp*, *sk*, *ht*, *ft*. These five classes of exceptions, and other exceptions, only serve to establish the law as the expression of a real force, and furnish most desirable data from which to work out the exact nature of the force. Setting them aside for the present, the great body of facts are plain and simple examples under the law; that is, as Grimm states it (*Geschichte der deutschen Sprache*, I. 276) :

the sonants	<i>d, b, g,</i>	change into
the surds	<i>t, p, k, c,</i>	which change into
the aspirates	<i>th, ph, f, ch, h,</i>	which change into
the sonants	<i>d, b, g.</i>	

If we think out the facts which changes like these imply, it will be clear that at least two sets of changes must have gone on together. Thus, when *d* changes to *t*, if the old *t*'s remain unchanged, there is no reason why the *t*'s which come from *d*, should have a different history from other *t*'s. But we find that words which at first had *d*, now have *t*; while

those which at first had *t*, now have *th*. It is certain that the originally separate and still separate sounds have never been merged into one. When *d* changed to *t*, the old *t* must have changed. Grimm supposed that all three sets of changes went on together. He thought the sounds to be held firmly apart like spokes in a wheel, so that when any one was modified, or moved, the neighbor in whose direction it moved, moved on, and in turn moved the third; and hence he called it *Lautverschiebung*. His theory of it is that the power which turned the wheel was applied to the sonants and gave them the utterance of surds; *d*, for example, was strengthened to *t*, the old *t*'s were moved on to *th*, and the old *th*'s to *d*'s.

Bopp, on the contrary, held it to be a weakening, beginning with the softening of the surds to spirants; *t* to *th*, and so on.

As more exact study of the early tongues went on, a manifold ambiguity in the so-called aspirates became plainer and plainer.

In modern Greek,  $\delta$  and  $\theta$ , and in English, *th*, *dh* are pronounced as spirants, with a simple continuous sound. In India it was found that the Sanskrit *tH*, *dH*, are pronounced as aspirates, nearly like the letters *t* and *h* in our compound *hot-house*, and *d* and *h* in *mad-house*. This sound we shall distinguish from that of the spirants by using a capital *H*. There has been, and is, division of opinion about the real sound of the Sanskrit. Brücke, Max Müller, and others, hold that the aspirate sound just described is impossible, or too difficult to be credible, for the sonants *dH*, *bH*, *gH*: that it must be an invention of the grammarians, and that the real sound was different, perhaps fricative like the German *pf*, *ts*. The set of opinion seems to be in the direction led by G. Curtius, who holds that the aspirate was the real sound. There is also division of opinion on the question whether the originals in the Parent Speech were like the Sanskrit. The Sanskrit has two sets of aspirates, surds *tH*, *kH*, *pH*, and sonants *dH*, *gH*, *bH*, and the German sonant mutes are found in words which in Sanskrit had sonant aspirates, while the corresponding words in Greek have surd. Kuhn and other

weighty authorities hold that the Parent Speech had surd letters, and that the Sanskrit sonants are weakenings from them. They rely mainly on the appearances to Greek, and the physiological argument that regular weakening must be from surds to sonants. Curtius, however, and with him the greatest number of eminent philologists, accept the sonant aspirates  $dH$ ,  $bH$ ,  $gH$  as the parent letters.

The corresponding letters are as follows :

Sanskrit	$dH$ ,	$bH$ ,	$gH$ .
Greek	$\theta$ ,	$\phi$ ,	$\chi$ .
Latin	$f$ , $b$ , $d$ ,	$f$ , $b$ ,	$h$ , $g$ .
Gothic	$d$ ,	$b$ ,	$g$ .

The Bactrian, Lithuanic, Slavonic, and Celtic are like the Gothic, so that a general set towards the sonants in the Indo-European tongues is plain. The change to surds in Greek and Latin is explained as an irregular assimilation of the sonant  $d$ ,  $b$ ,  $g$ , by the aspirate  $H$ . The common movement is a natural weakening by dropping the  $H$  of  $dH$ ,  $bH$ ,  $gH$ , which are very difficult sounds to make, and hence originates a very early and very strong tendency to change ; and this, they say, is what turns the wheel and moves all the other letters. This is the theory of Curtius. The prevailing argument for it is that the great majority of the languages are sonant. The Parent Speech being a hypothetical language deduced from the sister families must be expected to agree with the majority.

If one with this thought in mind examines closely the condition of this majority, he is easily led to a fourth theory. It will be seen that in other languages than the Sanskrit, Greek, Latin, and Teutonic, one set of letters, the sonant mutes  $d$ ,  $b$ ,  $g$ , answers for the two sets  $d$ ,  $b$ ,  $g$  and  $dH$ ,  $bH$ ,  $gH$ . In the language of the three theories already given, the original aspirates become merged in the sonants ; but may not these numerous families without the aspirates represent most truly the Parent Speech, and the cultivated families be the ones that have varied from it ? It is the opinion of many phonologists, of Mr. Ellis, for example, and Professor Haldeman, that the Sanskrit aspirates, at least in any pronunciation like

that given them in India, must be dialectic, and are probably late. This opinion would seem to demand some such hypothesis as our question suggests. But grave difficulties arise when we attempt to think it out. If we begin, for instance, with the *d*'s of the Gothic, Slavonic, Celtic, and the like, and suppose them to be the original *d*'s of the Parent Speech, we must then suppose that the Sanskrit, Greek, and Latin have changed a part of their old *d*'s to *dH, θ, f*. This might be easily believed; but the same words are changed in all three of the languages. This agreement cannot be accidental. There must have been historical connection between these languages when or after the change took place; that is to say, the change must have taken place in the Parent Speech of these three tongues. If it be suggested that the Parent Speech of these three is younger than the Indo-European Parent Speech, and that the change may not go to an earlier common ancestor, a further examination of the Gothic will be necessary. This will show that while the supposed *d* of the Parent Speech is retained in many Gothic words, in many others it has changed to *t*, and it has changed to *t* in just those words which in the other languages retain *d*, and retained *d* in just the words which they change. This again implies historical connection. It brings the Germanic languages into the same inner family with the Sanskrit, Greek, and Latin; that is to say, it implies that the threefold distinction now existing and indicated by *t, d, th* in English must have existed in the Parent Speech of these four families at least.

It is indeed possible to say, and to believe if there were evidence, that this threefold distinction was first established in one tribe, or one locality, and thence extended as a matter of foreign influence to sister tribes or adjacent localities with which there was much intercourse. The effect of such intercourse has not been much studied. We are coming to it now as the dialects of Germany and England are examined more minutely, and the effects of it promise to be found considerable, and most striking in the modification of pronunciation.

It is also possible that a people might make all these different sounds of each organ, but not discriminate them. There are

many modifications of the *a*-sound in English which we do not notice. Some quality or qualities of a letter are fixed upon as characteristic, the other qualities are not regarded. The Chinese make letters on the ground of pitch and emphasis, but they are indefinite for us. So it is with sonancy among the Finns and Polynesians. It is not a point which they notice, *p* or *b* is all one to them, since the lips move in the same way.

Beginning with a language in which *t*, *d*, *dh* are indefinite, it is possible to imagine the speakers of it growing to distinguish the different sounds and attach them to different words in such a manner that one tribe should use *t* where a second used *d* and a third *dh*, and so set the spokes in their wheels differently from the first. It might come about in the following fashion : The fact that one man and his village uttered his letters with more sonancy than another would attract attention first in a few striking examples in familiar words. When attention was once awakened to this difference, a sort of shibboleth would be made of it, and it would soon be perceived that there was a difference among words as pronounced by the same man, like this difference made in the same word by different men. The discriminated pronunciation of a considerable number of words would then be rapidly fixed under the guidance of two ideas—first, that the families or tribes are to differ in their pronunciation of each word containing these heretofore indefinite sounds ; second, that the words are to differ from each other in the shibboleth sound. An American has only to remember how Irishmen change the sounds of new words which they learn from us, to perceive that these ideas must have been considerable forces in the history of dialects. The working of them out in the case we have supposed, would gradually lead, if we give enough action and reaction between the tribes, to the formation of classes of words in all the dialects, containing the same words in the corresponding classes, but having each word differently pronounced,—a state of facts similar to that in the Sanskrit, Low German, and High German now. This line of thought has been suggested by Professor Max Müller's

famous account of Grimm's Law, and agrees with it in supposing that the mutes now found in Low German and High German are not to be explained as changes from those occurring in the corresponding words in Sanskrit, Greek, or the Parent Speech, but that the three sets are of equal age, and were gradually worked out of an earlier indefinite pronunciation by the influence of each on the others.

A hypothesis which makes the separation of High German from Low German wholly coördinate with that of Low German from Sanskrit, and of so high antiquity, is hardly credible on general historical grounds, aside from the linguistic difficulty of so complex a movement as this threefold simultaneous discrimination supposes. Little, if any, notable assent has been given to the doctrine of the primæval separation of Low German from High German, or indeed to any such views as those last set forth.

Mr. Sweet, at any rate, accepts the Low German letters as a change from those of the Parent Speech, and the Low German change as prior to the High German. He also accepts the current view of the sounds of the letters in the Parent Speech, and believes that the Low German sonants come from the aspirates *dH*, *bH*, *gH*. He agrees also that the three sets of letters in the Parent Speech have never been mingled, and that when, e. g., the old *d*'s changed to *t*'s, the old *t*'s must have changed to some other letter. But instead of the wheel movement, he supposes a criss-cross, an interchange; *t* and *d* exchange places; when *t* goes to *d*, *d* goes to *t*, and *dH* remains unchanged. His scheme for the linguals is as follows:

Parent Speech	<i>t</i> ,	<i>d</i> ,	<i>dH</i> ,	change to
Oldest Teutonic	<i>d</i> ,	<i>t</i> ,	<i>dH</i> ,	which change to
Oldest Low German	<i>dh</i> ,	<i>t</i> ,	<i>d</i> ,	which change to
Oldest High German	<i>d</i> ,	<i>tH</i> ,	<i>d</i> , <i>t</i> .	

What is called Oldest Teutonic is not found in any documents, but is a hypothetical Teutonic Parent Speech, made out by Mr. Sweet on the ground of the probability of the changes, and supported, as he thinks, by certain facts in Gothic and Anglo-Saxon.

We will first examine the theoretical probability of the changes.

1. The starting point of difference between this scheme and that of Grimm or Curtius, is in the changes of the original *t*. This they suppose to change to *tH, th*, the last being the sound which we know it to have in English, and which they suppose it to have had in Gothic. Mr. Sweet gets from the known *t* in Parent Speech to the known *th* in English by inserting two doubtful steps, his *t* changes to *d*, then to *dh*, then to *th*. It is plain that Sweet's hypothesis is theoretically the less probable. The change of *t* to *th* is a natural weakening and presents no difficulties whatever. It is true that it is not so common as the change of *d* to *dh*. The sonant mutes have an incipient vowel murmur which makes it more common for them than for surds to relax the closeness of their stop, and so change into spirants. We know no changes of surd mutes to their spirants so wide spread as that of the Greek  $\delta$ , now always *dh*, and the similar changes in Spanish and Danish. Sweet's change from *t* to *d* is also a common weakening, so that his run from *t* to *d* and from *d* to *dh* is easier than Grimm's from *t* to *th*; but it is a fatal facility, which carries him beyond his proper stopping place at *th*. To rise from *dh* to *th* is against the laws, and makes the whole hypothesis in so far improbable. It may be said that we have *dh* also in the English pronouns, and elsewhere, and that Grimm's explanation of that must be compared with Sweet's explanation of *th*. We have then to compare Grimm's series *t, th, dh*, with Sweet's series *t, d, dh, th*, where the first is all regular weakening, and the last has to rise from *dh* to *th* against law. It should be added that any difficulty with a change from *t* to *tH* is removed for Mr. Sweet by his admitting it to form part of a similar system of changes,—that from Low German to High German.

2. The real difficulty in Grimm's Law, the change of sonants to surds, of *d* to *t*, is not only not removed, but heightened tenfold by supposing it to occur at the same time as a change from *t* to *d*. Such interchanges when made by individuals belong to morbid speech, to aphasia. It is doubtful if any

real example of it is to be found in the speech of a nation. Mr. Sweet compares it to the cockney's *h*'s. The facts about that are not well known. Mr. Ellis seems to be at a loss about it. That *h* is easily dropt is well known. That persons who generally drop it should sometimes sound it, and that in the wrong place, is not strange, and perhaps that is all there is in the matter. The Armenian is also sometimes said to have interchanged two sounds since the early period; but such a change can be believed only on the strongest evidence, and perhaps only when mediated by other changes. Careless or unskilled hearers sometimes mishear strange sounds into these interchanges. English Americans here say that Germans pronounce *w* as *v* and *v* as *w*. The German *w* (*bh*) has not the semi-vowel sonancy by which we recognize *w*, and when they pronounce an English word containing *w*, it sounds strange, and we emphasize the strangeness unduly, and say that they make a *v*. The same German sound (*bh*), when made for *v* in an English word, is strange there too. The German makes it by closing with his upper lip, instead of his upper teeth. But the sound of the teeth is the characteristic quality of *v* to us, and so we say that he makes a *w* for *v*. And it is true that such a mishearing might possibly lead to a real interchange of letters.

3. The putting off the change from *dH* to *d* till the Low German period is improbable and unhistoric; improbable because, as has before been pointed out, *dH* is so difficult a combination to utter and therefore so likely to be unstable; unhistorical because we know that this combination actually gave way so early all through the Indo-European tongues as to antedate our earliest memorials in every tongue but the Sanskrit.

4. An examination of the supposed changes to High German does not add to the probability of the scheme. The High German *d* is reached by going first from *t* to *d* then to *dh*, then back again to *d*. The advancing backwards has a paradoxical air, which Grimm's *t* to *th* to *dh* to *d* is free from.

It would seem therefore that Mr. Sweet's theory is inferior in theoretical simplicity and antecedent probability, and would

never have been put forward, or perhaps seriously entertained, if the facts had not seemed to Mr. Sweet to necessitate some such hypothesis. We will now look at these facts.

They mainly relate to the earliest form of the lingual spirant as it appears in Gothic and Anglo-Saxon, and they serve to convince Mr. Sweet that it was one uniform sonant *dh*. His real reliance is, probably, on the appearances in Anglo-Saxon, but he begins with the Gothic. There, he says, "the thorn (þ) is uniformly represented by one simple character taken from the old Runic alphabet. This fact, taken in connection with the remarkable accuracy of Ulphilas's alphabet, makes it probable that the sound was also simple and uniform: either *th* or *dh*. A strong argument in favor of the latter pronunciation is afforded by the frequent and, in many cases, apparently arbitrary change between this *p* and *d* in the middle and at the end of words." Mr. Sweet, here as elsewhere, deals with facts in an off-hand fashion, which is natural for a busy man who is very familiar with the subject, but which makes things appear much more plainly on his side than they really are. Ulphilas has a character for a lingual spirant. It looks very much like a Greek ψ, which some of the students of such matters think it is; but it may be a rune. Ulphilas's alphabet is a very commendable effort in a phonetic way, considering where, when, and by whom it was made; but it is bold indeed to say that any one knows enough about its real accuracy to offer any assurance that it would have distinguished the two sounds of our English *th*. Eminent phonologists dispute about the value of nearly every character in it. Moreover, it is by no means agreed that his ψ is the only character he uses for a lingual spirant. It is a common opinion that the Greek δ had already begun at least to have the pronunciation of the sonant spirant *dh*, and that Ulphilas, like the Greeks, used one character, his *d*, for both the mute and spirant; so that, according to this view, the changes between *p* and *d*, are similar to the changes between the two sounds of the English *th*. And then again the fact that there are presumptions in favor of a sonant sound of *p* in the middle and end of words and not at the beginning, instead of being

an argument of any strength in favor of a sonant sound everywhere, affords a strong presumption against it, as we shall see.

There are a considerable number of words in which Teutonic *d* appears for *t* of the Parent Speech, instead of the regular *th* or *dh*. This *d*, Mr. Sweet tells us, is the original sound. He gives no proof. There is strong evidence of the contrary. The examples have been collected by Lottner in his well-known article on the exceptions to the first *Lautverschiebung*, in Kuhn's *Zeitschrift*, XI. 161, and it appears :

1. That many words which have *d* in Anglo-Saxon or later dialects have the regular spirant in Gothic, while the cases are very rare and doubtful in which a *d* is found in Gothic and a spirant in Anglo-Saxon or later dialects. This fact needs no comment.

2. Such *d*'s are found in the middle or at the end of words, and often in connection with sonant liquids, *l*, *r*, or *n*; they are not found initial, unless, possibly, in two or three words in connection with a sonant liquid: Latin *traho*, Gothic *dragan*, drag; perhaps *rpēπω*, *drive*. It is plain that being in the middle of a word, between two vowels, will have a tendency to convert surds into sonants.

This vowel assimilation is familiar in English and elsewhere in the change of *s* to the sound of *z*, as in *houses* from *house*; and of *th* to the sound of *dh*, as in *heathen* from *heath*. The same position has also an obvious tendency to convert mutes into spirants. The spirant differs from its mute by not closing the organs to so tight a stop. Vowel assimilation works just that effect. In modern Danish and Icelandic, *d* medial and final is regularly pronounced *dh*. Mr. Sweet's hypothesis begins with *d* everywhere, initial, medial, and final; and then supposes that the initial *d*'s changed to *dh* while the medial and final remained mute. Surely that is all wrong. If there is any going to sonant spirants, the place for it is the medial and final. The old hypothesis which supposes the surd aspirate or spirant *th* to be the original letter, and this to be changed to the sonant *dh*, *d* in the middle or end of words by vowel assimilation, has law on its side. It is also strongly supported by a set of changes in the inflection of the Anglo-Saxon verb.

The verb *cwæd* (þ) is inflected : singular *cwæd*, *cwæde*, *cwæd*, plural *cwâdon*; the verb *waes* has singular *wæs*, *wâre* *wæs*, plural *wîeron*; and other verbs the like ; where it is plain that the change of *d* (þ) to *d* is analogous to that of *s* to *r*, a surd to a sonant by vowel assimilation.

In Latin also there are similar adjustments of letters ; the original *dH*, *th* appears as *f* initial, but as *d* medial ; and so with *h* initial and *g* medial. As to which the probable theory (Ascoli, *Corsi di Glottologia*, I. 171) is that *dH* first became surd as in Greek, and then softened to the sonant when medial. Curtius also gives examples of media from aspirates in Greek, *Grundzüge*, 461, 468.

Again there are in Gothic some words in which *d* and *p* vary. This occurs in words whose proper letter is *d*, which regularly changes to *p* when final or before *s* in inflection. In the combination *ps*, *p* must be surd like *s*. Ulphilas has a separate character (z) for the sonant of *s*. In some other words there is varying spelling ; perhaps there was varying pronunciation, but it is true that the spelling shows that *d* and *p* were probably sometimes used for like sounds.

In modern English we have some exact knowledge to reason from ; *th* is generally surd when initial and often when final, and sonant when medial. The natural weakening is from surd to sonant. That our surds are changed from sonants, though not impossible, needs the best evidence before it can be received. Here Mr. Sweet's studies of the Anglo-Saxon manuscripts come to the front. It had been the current notion that the confusion of *d* and *p* in the best known manuscripts was due to late and careless scribes, and that if we could get really old and really careful manuscripts, we should find the letters uniformly discriminated. Mr. Sweet has been studying manuscripts carefully written for Alfred, and others of equal or nearly equal antiquity and authority, and finds that each uses a single character, one *d*, another *p*. He naturally, and confidently (and that is natural too) concludes that there was only one sound. That this sound was *dh* seems to have been impressed on him most by the shape of the written characters. One of them (*d*) is obviously a crossed *d*. The other (*p*) is

a rune, and Mr. Vigfusson's theory about it is, that it is a Latin d with the stem prolonged both ways. He thinks the runic sign for d was made by joining two d's back to back. Mr. Sweet accepts the suggestion of these forms, i. e. that original t was then pronounced d, and original dH as double-d. That comes pretty near being a merger of the two sets of letters. One would hardly expect d and double-d to have separate characters. But there is some weight in these suggestions. Exactly how much, I do not know enough about the history of alphabets to say. But the history of opinions about runes and Gothic characters and Anglo-Saxon characters is such, that even Mr. Sweet's conclusions about the matter do not much win my confidence—that plant of slow growth. To speak right out, Mr. Sweet's statements about the Anglo-Saxon manuscripts lack something of completeness. It would be comfortable to know exactly how many and what manuscripts use ð alone, and þ alone, and how many and what have both. The two Pastoral manuscripts are mentioned as using ð alone, the Parker manuscript of the Chronicle is mentioned as using þ alone. The Lauderdale Orosius shows both. These are all Alfred manuscripts, and here the specification ends. "The more accurate of the later MSS.," it is added, "generally write þ initially and ð medially." Mr. Sweet cannot really expect any one to rate this graphic evidence very high till it is much more fully set forth. A more solid source of evidence is found in words where these characters appear in combination with other consonant symbols, or change to other letters. Mr. Sweet says, "In the very oldest MSS. the words which have d, b, and g, instead of the later ð, f, and h, are so numerous, that we are almost forced to the conclusion that at a period not much earlier than the beginning of the eighth century, the sounds represented by ð, f, and h did not occur anywhere but initially." To this it may be said, first, that Mr. Sweet should state more definitely what manuscripts he counts as these very oldest. Does he mean a few scraps like the nine-line fragment of Caedmon? Then suppose it be admitted that at the beginning of the eighth century it is the common rule to find in place of the surds t, p, k, initial spirants, and

medial sonant mutes, the question arises which is more probable, that there were at first sonant mutes everywhere and that they have changed to sonant spirants when initial, or that there were at first surd spirants everywhere which have changed to sonants when medial. We have already pointed out that sonant spirants would be most likely to appear in the middle and not at the beginning of words. (See over, p. 91.) It may be said further that we have plenty of carefully written Low German a good deal earlier than the eighth century—the Gothic of Ulphilas of the fourth century—and so far from its being all medial *d*'s, there are more medial spirants than in Anglo-Saxon. To accept *d* as the original letter is to place the Gothic latest of the languages. Mr. Sweet's suggestion makes the Anglo-Saxon of the period a little before the eighth century indefinitely more primitive in its consonant system than the Gothic of the fourth and, it may be pretty safely added, than the general Teutonic status of the fourth. For we get to High German from *t*, according to Mr. Sweet, through *d*, *dh*, to *d*; and High German had already reached the second *d*, at or about the time that he places Anglo-Saxon at the first one. That Anglo-Saxon is not such a primitive speech in most respects is certain, that it is so in this respect is antecedently very improbable. So much for Mr. Sweet's hypothetical *d*.

Now for the question whether the manuscripts of Alfred's time establish one only uniform sonant spirant (*dh*).

Mr. Sweet's scheme is :

	Initial.	Medial.	Final.
Before 8th century,	<i>dh</i> ,	<i>d</i> ,	<i>d</i> .
Alfred's reign,	<i>dh</i> ,	<i>dh</i> ,	<i>dh</i> .
Later Anglo-Saxon,	<i>th</i> , <i>dh</i> ,	<i>dh</i> ,	<i>dh</i> .
Modern English,	<i>th</i> , <i>dh</i> ,	<i>dh</i> ,	<i>th</i> .

But the laws of utterance make it certain that there was a surd spirant in the time of Alfred. The *d* (*p*) occurs in combinations where it must have been surd. The third personal ending of the verb is found abundantly in syncopated forms after surds: such are *drincd*, drinketh; *cryptd*, creepeth, and the like. A sonant spirant never was pronounced in these

combinations. Mr. Sweet himself, on page 501, has his little thrust at the want of capacity of "modern critical editors, who do not stop to consider whether their 'normalized' *spricð* *pirsedð* (-skdh), &c., are phonetically possible or not." But there are plenty of them in his text. To these regular examples may be added as somewhat peculiar, those in which a *t* remains, as *restð* (*resteth*), or is inserted, as *gecistð* (*chooseth*), *geristð* from *gerisan*; where the surd sound is certain, though the inserted *t* may be fairly said to testify that final *ð* was often sonant.

Another frequent combination in these manuscripts that Mr. Sweet has published, is *sð*, as in the second person of verbs, *tældesð*, blamedst; in superlatives, and elsewhere, abundantly. Mr. Sweet describes it at length as one of the most noteworthy objects in his text and admits it fully to be a voiceless or surd *th*. There was then a surd spirant in the time of Alfred represented by *ð*, p.

There are several conditions in which the common *ð* is changed to another letter, which seem to offer clear proof that *ð* (*y*) was surd: *bit* for *bided*, *bint* for *binded*, *tret* for *tredeð*, and the like, strongly imply a surd *ð*. How else should a surd *t* arise from *dd*?

Indeed in this manuscript we find an uncontracted *-et* not infrequent for *-ed*: *dyncet* for *dynced*, thinketh, and see other examples on p. xxxiv. This evidently is to be put with the syncopated changes just mentioned, and these with the Gothic law of final combinations, and so the surd character of the *ð* is grounded in the depths of Old Low German (March's Comparative Grammar of the Ang. Saxon, p. 97). It is a little remarkable, in view of Mr. Sweet's special advocacy of this uniform sonancy, that his manuscripts show so many facts which contradict it, or look the other way, and not one which favors it, except the uniform sign *ð*. Thus there are no examples in them of irregular medial *d* for *ð*, no verbs ending in *-ed* for *-ed*, no irregular examples of syncopated verbs with other sonants for *ð*, or anything of the kind. The special forms are all against uniform or predominant sonancy. Mr. Sweet, to be sure, gives syncopated *-ed* as one of his verb

endings ; but the examples are simply dropt *d*'s after roots ending in *d*: *gewend* from *gewendan*, *gefréd* from *gefrédan*, p. xxxiv.

It appears then that Mr. Sweet's deductions from the uniform sign *d* in his manuscript were hasty. The phonetic laws show that there were both surd and sonant spirants. Put this fact with that of the uniform sign, and it becomes nearly certain that the early scribes did not distinguish the surd and sonant sounds, any more than our scribes now do in English. The same was true of *f* and *s*. The spirants are from their mode of formation especially sensitive to the influence of other letters,—of the vowels, because they are a sort of incipient vowels, and of other consonants, because they do not wholly stop the breath, and can readily combine with other sounds. In most languages we find their spirants oftener sonant between vowels than at the beginning of words. The difference between the surd and sonant sounds of any spirant is slight compared with that between the surd and sonant sounds of a mute. The spirant never has the sharp closure of the mute surd. Hence most languages have not distinguished the surd and sonant sounds of the spirants so carefully as those of the mutes; *s* is the most universal spirant, and one character for it generally does duty both as a surd and a sonant consonant, so does *th* in English, so did *f* in Anglo-Saxon, and so did thorn in Gothic and Anglo-Saxon.

That this thorn began predominantly surd and has been changed to sonant by vowel assimilation in the middle and often at the end of words appears from the following facts now briefly recapitulated :

1. Gothic *p* and Greek *θ* transliterate.
2. The Gothic change from *d* to *p* before *s* indicates a surd *p*.
3. In the transliteration of Gothic names into Latin, we have at first *th* for *p*, then gradually *t* comes to be used at the beginning, *d* in the middle of the names.
4. The earliest Ang.-Sax. MSS. represent the spirant by *th* as they do the Greek theta.
5. They also show a difference between initial and medial

positions by the uniform spirant initial, the frequent sonant mute sign medial.

6. Frequent Ang.-Sax. combinations of letters, e. g. *sðt*, *stðt*, *cdð*, *pdð*, indicate a surd *ð*: and so do the results of several phonetic changes where *ð* gives rise to *t*.

7. In a large part of the good Ang.-Sax. MSS. there is a visible inclination to use a different character (*p*) at the beginning of words from that (*d*) used in the middle and at the end.

8. The English language uses surd *th* initial, sonant *th* medial and often final, with certain exceptions—mostly pronouns and inflection endings which have a peculiar history.

9. The place for a sonant spirant to first appear is in the middle of words, not at the beginning as Mr. Sweet's theory would have it.

The other sets of letters go with the linguals. Mr. Sweet says in his summary way, "There can be no doubt that the *f* was originally vocal in all cases, like the Welsh *f*." When one observes the facts in Gothic expressed by the rules, "*p* before *t* changes to *f*," and "*b* before *t* changes to *f*," and observes that *ft* and *fs* are common combinations, he will be sure that *f* was not vocal in all cases in the oldest Germanic speech of which we have any direct knowledge. An examination of the whole matter gives a series of facts generally similar to those set forth in regard to *th*, *dh*, and has heretofore satisfied the ablest philologists and phonologists that *f* was originally surd, nor is any reason obvious why it should not do so in the future.

Our study of the theoretical simplicity and probability of Mr. Sweet's criss-cross hypothesis for Grimm's Law led to the conclusion that its difficulties were so great that he must have been driven to it by some supposed compulsion from the facts. Our study of the facts leads to the conclusion that they are irreconcilable with his hypothesis about the original uniform sonant spirant; but if that be given up, his whole hypothesis about Grimm's Law is objectless and baseless.

Meantime there has been within the last few years in Germany a constant succession of essays on the *Lautverschiebung*.

As the study of living speech in physiology and in the dialects begins to bear fruit, each of the new men naturally has his word to say about the great German linguistic problem, or some of its corollaries ; oftenest, of course, about the relations of High German and Low German, which are both still alive.

In the first setting forth of Grimm's Law it was said that the Low German and Scandinavian tongues remained in the state to which a single application of the law to the Parent Speech would bring them, while High German repeated the operation. The High German letters are said to bear the same relation to the Low German, that the Low German bear to the Sanskrit.

A closer examination of the facts shows a large number of exceptions to this rule. Indeed, if the prevailing forms in Old High German be accepted as Old High German, only the linguals *t*, *d*, *th* answer to the law. The other letters under the law are unchanged, except that *k* changes to *ch*.

It is to be noticed, however, that a part of the ancient documents (7-11 century) write surd mutes for all the sonant, i. e. *k* for *g*, *p* for *b* as well as *t* for *d*. According to Graff's examination of them, 8 write *k* alone ; 150 mingle *k* and *g* without rule ; and 110, including Isidor, Otfried, Tatian, Williram, have exclusively *g* ; 39 have *p* for *b*, all glossaries and fragmentary except 3 ; 49 retain the old *b* throughout ; all the rest mingle *p* and *b* without rule. A closer examination shows a geographical ground of classification. The writers bordering on Low German retain the old letters. The *Oberdeutsch* have the surds in their earliest writings, and the traditional pronunciation of the region is still the same. We may therefore infer that the cause or causes which produce the Grimm's-Law changes were really at work, but the influence of the Low Germans finally carried the day in the literary and court speech. Grimm, Geschichte d. S., 424. The aspirates or spirants only do not change, *h* and *f* remain the same. This bears hard on the theory of Curtius that the force which turns the wheel of sounds is the gravitation of aspirates. There is certainly no such force in the High German shifting of labials and gutturals.



The minute study of the physiology of speech, and of living dialects, has led to the making of distinctions among sounds much finer than have been recorded in any alphabet. These minuter distinctions enable the phonologists to break up Grimm's wheel; they show how sounds may be kept separate which would be mingled, so far as the Roman letters go. There are, for example, different degrees of sonancy given to *d*, *b*, *g* in different parts of Germany; so, too, the stress or volume of breath with which the surd *t*, *p*, *k* are uttered varies much. Americans who live in regions settled by Germans are continually made aware of these facts. There is so little sonancy in the common German *d*, *b*, *g*, that they sound to us like *t*, *p*, *k*. I have just now, while writing about this thing, given my German American neighbor an order for a *peck* of grass seed, and I find it is a *bag* that he wants. These sonants of little or no sonancy are called whispered letters. We notice that *k* has a peculiar sound, and on attending to it, we hear or seem to hear *kH*. If *d* were to move to *t* in this way by dropping sonancy, it would still be distinct from the old *t* with stress, so that we should not need the wheel hypothesis to keep them apart. Some of the later Germans have accordingly dropt it, and advanced theories explaining the changes as having taken place independently and even at different times. Of these, one of the most lively, and suggestive, and many-sided and provoking, physiological, psychological, and poetical as it is, is that of William Scherer in his *Zur Geschichte der deutschen Sprache*. The chief source of the peculiarities of German speech, he thinks, is to be found in the change which the social conditions after the occupation of Germany brought about. These raised passion, the heroic ardor of the warrior, to a dominant energy. This led the poets, the creators and shapers of speech, to aim at emphasis and passionate strokes. Hence alliteration; hence also a change in the nature of accent from pitch to stress. Feeling is expressed in the vowel sounds. Hence arose a finer modulation of the vowels and more stress upon them. But more stress on the accented vowels implies less on the consonants, and lightening the consonants is the essence of

the *Lautverschiebung*: it weakens surds to spirants, spirants to sonants, sonants to whispered letters. The change from whispered letters to true surds he attributes to alliteration. This preserves the old distinctions among initial sounds and even augments them. A part of the growing stress was led to this form of utterance by the attention being so much drawn to initial letters by their use on the lots employed in divination.

The cause of the second or High-German shifting he finds in Romanic influence on the High Germans, especially in the influence of the folk poesie and church hymns on the poets and poetical forms of the High Germans. He does not work out the particular applications of this thought. He goes fully into proximate physiological causes, not only of the general movements, but of the exceptions.

He begins with the *surd mutes*, and gives the following rules for them: Initial and after liquids they change to surd affricata, i. e.,

<i>p</i> ,	<i>t</i> ,	<i>k</i> ,	change to
<i>pʃ</i> ,	<i>tʃ</i> ,	<i>kh</i> .	

Between vowels they shift to surd spirants:

<i>p</i> ,	<i>t</i> ,	<i>k</i> ,	change to
<i>f</i> ,	<i>z</i> ,	<i>hh</i> .	

Next the *spirants*. These should go to sonant mutes, and only those could go which had become sonant at the time of the general shifting. The lingual alone was ready, *th* having become *dh*, and it alone shifted to *d*. Why it had become sonant he does not know, but he compares it with the change of *s* to *z* in English. Last the *sonant mutes*. These, he thinks, changed by dropping their sonancy; the real sound became a whispered sonant rather than a true surd. The labials and gutturals had no well-uttered sonant and so needed to make no change to distinguish the whispered one from it. But in the lingual series a full sonant arose from *dh* and appropriated the character *d*, and then the whispered *d* was denoted by *t*.

These movements he regards as independent. His attempt at a solution of Grimm's Law is by explaining each movement as a natural weakening. The combination he leaves to chance.

And the problem is still unsolved. We are interested in the subtle analysis which brings out increments of motion slighter than letters had recorded, but, after all, the letters do move in different directions on the line of easy utterance. The letters have a different history here from that which is found elsewhere, and the repetition of shifting makes a plain demand for some permanent forces to explain it. What is wanted is to postulate some peculiar tendency in this speech which, when acting together with the general laws, will, in the known conditions of the German letters, give resultants at every step of the history such as we find to have in fact appeared. This is what Curtius has attempted by postulating a special German tendency to preserve all distinctions in speech. The attempt to combine this tendency with the regular weakenings so as to make the sounds change places like the turning of a wheel, lifting up one set of spokes by the weight of the others, has much the effect of tracing the power in some ingenious machine for producing perpetual motion. But the working of Grimm's Law does not leave the same sounds in existence. When a second shifting has been carried out, as in the *Oberdeutsch* dialects, the aspect of the tongue is changed. There is but one sonant left of the original six, and that comes by good luck from a superfluous change of *th* to *dh*. There seems to be wanted as the postulate a tendency to some change in the kind of sounds. And if we proceed in Newton's manner, framing no hypothesis, but generalizing a fact and treating it as a power, the postulate is before us, that the Germans have a special tendency to give up sonant consonants. To establish this as a power, we need to see if we can find proof of it, outside of the facts from which we took it, and then define with scientific precision the modes or laws of its working, and its historical development. As to facts, an American may convince himself at any time that Germans habitually use less sonant breath than he does in making what is intended to be the same letter. In trying to pronounce English, the German seems at first to convert each letter according to Grimm's Law : a closer observation shows that he uses less sonant and more surd breath, and that his *t*

and *d* are distinguished to the ear, not by sonancy and the want of it, but rather by different volumes of surd breath. It is well known to students of these matters what difficulty the great German scholars have about accepting sonancy as the distinctive quality of what they call medial, or soft letters.

As might be expected from these statements, many persons and even dialects in Germany never distinguish the surd from the sonant mutes. The manuscripts show that it has always been so. And, in particular, the facts on this point given from Graff, on page 98, show that before the establishment in the schools of the second *Lautverschiebung*, there was general confusion among the High Germans as to surds and sonants. Our train of thought suggests that this confusion is a condition towards which the German tends more strongly than other Indo-European races: there are outlying races who have no sense of this distinction. How then can the *Lautverschiebung* be explained? I ventured the suggestion in my Anglo-Saxon Grammar that it may be a matter of foreign influence. This also was a generalization of a case where we know it to occur, that in which an Englishman undertakes to represent a German's pronunciation of English. According to this thought the typical German, left to the operation of the tendency against sonants, falls, or rises, into a state in which the old surds and sonants are distinguished by different distinctness. When he now comes in contact with other nations who have retained the old utterance of the surds and sonants, they hear his unsonant sonants as simple surds, and his vigorous old surds as aspirated surds, and pronounce his sonant aspirates or spirants as simple sonants. From some historical reason he is educated by them and accepts their interpretation of his speech. The whole of the German tribes thus raised themselves by early collision with the Europeans. The old High Germans, settled for a long time out of reach, passed again from the Low German position into the confused condition in which they are found in our earliest manuscripts, and a second time attained the old discrimination, so far as they have attained it at all, under the joint influence of the Romans and Low Germans.

VIII.—*Vocabulary of the Language of the Indians of San Blas and Caledonia Bay, Isthmus of Darien.*

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Headquarters Nicaragua Surveying Expedition, }  
Castillo Viejo, Nicaragua, May 9, 1873. }

The vocabulary herewith forwarded to the American Philological Association, was obtained while I was attached to the Darien Exploring Expeditions of 1870–71, and mostly through the medium of the Spanish,—many of the Indians speaking that language with some degree of fluency. I took every opportunity that was offered for verifying it, and I have full confidence in its correctness. I have marked all words that I regarded as at all doubtful.

The system of numbers will, probably, be clearly enough exhibited, by the series I have given. The numbers run from one to ten, then by  $10 + 1$ ,  $10 + 2$ , and so on, to twenty—which has a name: then,  $20 + 1$ ,  $20 + 2$ , etc., to thirty, which is  $20 + 10$ . Thirty-one is  $20 + 10 + 1$ ; forty is two twenties; one hundred is five twenties; and two hundred has a name.\* One thousand also has a name.

There is no plural form for nouns.

The vowels have the same sounds as in Spanish, and all are to be pronounced, excepting only *aw* diphthong, which has the sound of English *aw* in *saw*, and *oo* which is sounded as in *boot*. The *u*, marked *ü*, is very short. *G* is always hard. *Ch* and *s* are interchangeable, as are the syllables *gue* and *gua*, with *we* and *wa*, and *ke* with *ge*. The Indians do not easily distinguish between *ch* and *s*, even in English words: they commonly say “a’chame” for “all the same.”

\* [The system is vigesimal. ‘Twenty’ is *tula-guena*, i. e. ‘one man’ (all the fingers and toes): 200 is *tula ’tala-ila-póguia*, i. e. ‘five-twenties by two’: 1000, *tula wala guena* ‘great one-man’ or ‘great hundred.’—J. H. T.]

The accents marked in certain words show the syllables on which the stress is laid — there being no general rule for accent. The accent sometimes changes its place when the word is compounded with another.

Respectfully submitted,

EDWARD P. LULL, *Commander U. S. Navy,*  
*Commanding Expedition.*

God,	<i>Dios</i> (Span.)	tongue,	<i>wabina.</i>
Heaven,	<i>neka-armake</i> , or <i>nia-</i>	teeth,	<i>nugala.</i>
	<i>dios.</i>	cheek,	<i>wagala.</i>
spirit, or mind,	<i>purpa.</i>	chin,	<i>akú.</i>
man,	<i>tulu</i> , or	beard,	<i>tsiga.</i>
	<i>matseridi</i> (=male).	neck,	<i>tukala</i> , or <i>gamugua.</i>
woman,	<i>ome</i> , or	shoulder,	<i>sakua.</i>
	<i>puna</i> (=female).	arm,	<i>harkaua.</i>
young woman,	<i>pundola.</i>	elbow,	<i>kasimuro.</i>
boy,	<i>machiqua.</i>	wrist,	<i>kapuspusigua.</i>
girl,	<i>pundqua</i> , or <i>puna'ló-</i>	hand,	<i>sargaua.</i>
	<i>gua.</i>	fingers,	<i>kogua.</i>
infant,	<i>kuarúqua.</i>	chest,	<i>kuage.</i>
my father,	<i>ani parba.</i>	heart,	<i>kuage.</i>
my mother,	<i>ani nana.</i>	stomach,	<i>pinagua.</i>
my wife,	<i>ani puno.</i>	belly,	<i>chabala.</i>
my son,	<i>ani machu.</i>	back,	<i>yerkoua.</i>
my daughter,	<i>ani chisqua.</i>	leg,	<i>tuqua.</i>
my elder brother,	<i>ani yagua.</i>	knee,	<i>yókoro.</i>
my younger brother,	<i>ani quenati.</i>	foot,	<i>nága.</i>
my sister,	<i>ani puna</i> (? doubt- ful).	toes,	<i>koprígua.</i>
my cousin,	<i>quenal'paraguadi.</i>	skin,	<i>uka-atarisa.</i>
my uncle,	<i>kilu.</i>	town, village,	<i>nekubro.</i>
Indian,	<i>Indio.</i> (No native word except <i>Tulu</i> 'man').	house,	<i>neka.</i>
head,	<i>sagila</i> , or <i>nonogua.</i>	roof,	<i>akwa-neka.</i>
hair,	<i>afsala.</i>	thatch,	<i>veroka-neka.</i>
hair of the head,	<i>sagil'afsala.</i>	chief,	<i>sagila</i> (head).
face,	<i>wakala.</i>	friend,	<i>aya.</i>
eye,	<i>ibia.</i>	comrade,	<i>mala.</i>
ear,	<i>oogua</i> (oo as in boot.)	bow,	<i>kinge.</i>
nose,	<i>azugua.</i>	arrow,	<i>warsiga.</i>
mouth,	<i>kaya.</i>	gun,	<i>kinge</i> , (see 'bow').
		cannon,	<i>kinge-tumadi</i> (great gun).
		pistol,	<i>kinge-pistoli,</i>

[*Machu-agua* 'young male'? Comp. *machu* "son": *matse-ridi* "male."]

axe, hatchet,	<i>akkaua.</i>	daylight,	<i>iskine.</i>
macheta,	<i>esa</i> , or <i>es'a-nori.</i>	forenoon,	<i>tata-apala</i> (-half-sun).
knife,	<i>es'tine.</i>	noon,	<i>tata-yórkowe.</i>
sword,	<i>esa-swite</i> , or <i>esa-punga-la.</i> ( <i>swite</i> = long.)	afternoon (early),	<i>tata-aipiníte.</i>
canoe,	<i>ulo.</i>	afternoon (late),	<i>tata-nate.</i>
ship,	<i>ulo-tumadi.</i>	sunset,	<i>tata-arquate.</i>
steamer,	<i>tsaw-ulo</i> (fire canoe.)	twilight,	<i>sétose.</i>
ship-of-war,	<i>peris-uló.</i>	darkness, night,	<i>mutige.</i>
boat,	<i>úrboda.</i>	to-day,	<i>émis.</i>
paddle,	<i>kami.</i>	now,	<i>emis-cua.</i>
mast,	<i>mor'-puarra.</i>	to-night,	<i>mutige.</i>
sail,	<i>ur'-mola.</i>	yesterday,	<i>ídye.</i>
oar,	<i>rémo</i> (Spanish).	to-morrow,	<i>pdne.</i>
rudder,	<i>sorgae.</i>	day after to-morrow,	<i>achuli.</i>
pipe,	<i>pipa</i> (Span.).	cloud,	<i>mogila.</i>
tobacco,	<i>wawa.</i>	rain,	<i>ti-wia.</i>
" smoking,	<i>wawa-chigaligua.</i>	wind,	<i>prua.</i>
" chewing,	<i>wawa-kwamakale.</i>	storm,	<i>máлага.</i>
basket,	<i>karpa.</i>	water,	<i>ti.</i>
shovel,	<i>memet.</i>	river,	<i>ti-wala.</i>
bag,	<i>sácu</i> (Span.).	bay,	<i>káka.</i>
net,	<i>mórtogua.</i>	sea,	<i>termala.</i>
barrel,	<i>pari</i> (Span.).	wave,	<i>termala.</i>
box,	<i>ulugua.</i>	surf,	<i>termala sere.</i>
money,	<i>mani.</i>	beach,	<i>ukupu.</i>
a present,	<i>pinche.</i>	sand,	<i>ukupu.</i>
seat, chair,	<i>kána.</i>	shell,	<i>mórpepe.</i>
hammock,	<i>katchi.</i>	small shell,	<i>tolu.</i>
blanket,	<i>móla.</i>	salt water,	<i>ti-palu.</i>
clothes,	<i>mála.</i>	good water,	<i>ti-nuerdi.</i>
hat,	<i>kurgena.</i>	hot water,	<i>ti-urquegua.</i>
shirt,	<i>yoaladi.</i>	deep water,	<i>ti-wila.</i>
coat,	<i>suba.</i>	land,	
pantaloons,	<i>calson</i> (Span.).	mountain,	<i>yala.</i>
shoes,	<i>sapatos</i> (Span.).	valley,	<i>mastigna.</i>
ring,	<i>arkan-yoedi.</i>	island,	<i>upa.</i>
rope,	<i>urduba.</i>	rock, stone,	<i>acua.</i>
north,	<i>yala.</i>	mud,	<i>oli.</i>
sun,	<i>tata.</i>	road, trail,	<i>ikala.</i>
moon,	<i>ni.</i>	wood,	<i>chuwarra, suarra.</i>
stars,	<i>niscua.</i>	tree,	<i>chápi.</i>
day,	<i>ibagine.</i>	pole,	<i>sua'swiliti.</i>
week,	<i>iba.</i> (doubtful.)	bush,	<i>karkana.</i>
half-month,	<i>ni-apála.</i>	root,	<i>mali.</i>
month,	<i>ni-quina</i> (one moon).	fire,	<i>tsaw</i> (aw as in saw).
year,	<i>pirka.</i>	smoke, subst.,	<i>gwa.</i>
morning,	<i>tata-naquiale.</i>	salt,	<i>palù.</i>
dawn,	<i>polisa.</i>	iron,	<i>es'koragua.</i>

gold,	<i>olo</i> (Span. <i>oro</i> ).	great, large,	<i>tumadi.</i>
copper,	<i>es'nuna.</i>	small,	<i>pipigua, senigua.</i>
corn (maize),	<i>opa.</i>	brave,*	<i>úrrowe.</i>
flour,	<i>purru.</i>	strong,	<i>kala-toga.</i> (See ‘much.’)
bread,	<i>mádu.</i>	weak,	<i>kalu-chuli.</i> (See ‘no.’)
chocolate,	<i>stagua.</i>	old,	<i>sereti.</i>
coffee,	<i>café</i> (Span.).	young,	<i>aguia, or logua.</i>
milk,	<i>maulé-nesa.</i>	good,	<i>nuerdi.</i>
meat,	<i>saua.</i>	bad,	<i>sterga.</i>
pork,	<i>china.</i>	dead,	<i>purguisa.</i>
rice,	<i>arros</i> (Span.).	alive,	<i>tula (?)†</i>
beans,	<i>inuwa.</i>	cold,	<i>tompe.</i>
bird,	<i>chike.</i>	warm, or hot,	<i>uguegua.</i>
chicken, or fowl,	<i>kaudra.</i>	amiable,	<i>aisè mala.</i>
egg,	<i>nablúluga.</i>	blind,	<i>aquidake.</i>
pelican,	<i>korke.</i>	sweet,	<i>ochigua.</i>
parrot, red-head,	<i>quaqua.</i>	sour,	<i>savole.</i>
“ yellow-head,	<i>arnagua.</i>	crooked,	<i>tópere.</i>
parroquet,	<i>quiliqua.</i>	straight,	<i>otikigua.</i>
feather,	<i>sakána.</i> (doubtful.)	dry,	<i>tenagua.</i>
wing,	<i>sarkana.</i>	deep,	<i>wila.</i>
<i>espíno.</i>	edible,	<i>nuerdi-masguine.</i> (lit. good to eat).	
horse,	<i>mauli</i> ; without dis-	fat,	<i>iavalu-toga.</i>
mule,	ting, not hav-	fresh,	<i>saegua.</i>
cow,	ing these animals	high,	<i>nigpa.</i>
	in use.	low,	<i>mataragua.</i>
leather,	<i>moluka.</i>	hard,	<i>séleba.</i>
cat,	<i>mize.</i>	soft,	<i>talaragua.</i>
dog,	<i>atcho.</i>	heavy,	<i>yagiticuwa.</i>
tiger,	<i>atcho-nigoria.</i>	light (of weight),	<i>tukulogua.</i>
snake,	<i>nagpe.</i>	lazy,	<i>wiye,</i>
fish,	<i>úwa.</i>	pretty,	<i>sunatagleje.</i>
alligator,	<i>taíma.</i>	poor,	<i>owresela.</i>
turtle,	<i>moróagua.</i>	quick,	<i>kwegua.</i>
banana,	<i>wamádduno.</i>	round,	<i>napoe.</i>
plantain,	<i>machi.</i>	broken,	<i>pichisa.</i>
orange,	<i>narasa</i> (Sp. <i>naranja</i> ).	ripe,	<i>koróquadi, quartigete.</i>
cocoanut,	<i>ógoba.</i>	rich,	<i>mani-ibedi, or mani-</i>
name,	<i>nuga.</i>		<i>toga.</i>
white,	<i>chipugua.</i>	savage, ugly,	<i>úrrowe.</i>
black,	<i>tisite.</i>	swift,	<i>arpamake.</i>
blue,	<i>tisite.</i>	slow,	<i>pinagua.</i>
red,	<i>kinite.</i>	long,	<i>swite, or chusite.</i>
yellow,	<i>ortéjete.</i>	short,	<i>giakwagua.</i>
green (color),	<i>turoti.</i>		
green (unripe),	<i>niraquagua.</i>		

\* Probably in the Spanish sense, i. e. aggressive. See, below, “ savage.”

† [Compare *Tulu*, man.]

sick,	<i>kekusa.</i>	to buy,	<i>paki.</i>
" very,	<i>diesuna.</i>	to bury,	<i>étuye</i> (cf. to shut).
sore,	<i>ya.</i>	to boil,	<i>kwarkwate.</i>
torn,	<i>esermake.</i>	to burn,	<i>kumakali.</i>
tame,	<i>kegurrowe.</i>	to build (make),	<i>sope,</i>
I, me, my,	<i>ani.</i> The final vowel is frequently dropped.	to build a house,	<i>neca-sope.</i>
thou, thee, thy,	<i>per.</i> " "	to cough,	<i>tolómake.</i>
he, him, his,	<i>atoni.</i>	to cut,	<i>chike.</i>
we,	<i>ani-pogua</i> (-I two).	to climb,	<i>ndkwe</i> (cf. to rise).
you,	<i>per.</i> (See 'thou').	to cook,	<i>mastowe.</i>
they,	<i>egrape.</i>	to capsize,	<i>aípine.</i>
equally,	<i>nayo-peragua.</i> (See 'same.'	to come,	<i>tage.</i>
enough,	<i>toga</i> (much).	" back,	<i>nolicowe.</i>
no more,	<i>percusa.</i> (See 'only.'	to chew,	<i>kwamakale.</i>
how many? how much?	<i>igi.</i>	to dig,	<i>miye.</i>
half,	<i>apála.</i>	to die,	<i>purkisa.</i>
less,	<i>abui-chuli.</i>	to dance,	<i>kwile.</i>
much many,	<i>toga.</i>	to drink,	<i>kóbe.</i>
several,	<i>chirpa.</i>	to eat,	<i>masquine.</i>
more,	<i>napi.</i>	to fall,	<i>árkuacha.</i>
only,	<i>cusa.</i>	to fire a gun,	<i>pani-okoloe.</i>
same,	<i>nayobi.</i>	to fill,	<i>énowe.</i>
soon,	<i>kwaégua.</i>	to find,	<i>armisu</i> (cf. to hunt).
too,	<i>tótraga.</i>	to fly,	<i>kukue-nae.</i>
this,	<i>ese.</i> (Spanish ?)	to go, or walk,	<i>nae.</i>
these,	<i>esa.</i>	go (future),	<i>naowe.</i>
where,	<i>pia.</i>	gone,	<i>náte.</i>
then,	<i>yópoba.</i>	to give,	<i>aukala.</i>
when,	<i>saua.</i>	to hunt,	<i>armiye</i> (cf. to find).
that,	<i>ese.</i> (Spanish ?)	to hear,	<i>kóle.</i>
up,	<i>nima.</i>	to have,	<i>nika.</i>
under,	<i>uláka.</i>	to kill,	<i>ibloge.</i>
upon,	<i>agilákine.</i>	to loose,	<i>echike</i> (Sp. <i>earchar</i> ?).
until,	<i>machuna.</i>	to leak,	<i>yearrowe.</i>
until to-morrow,*	<i>páne-machuna.</i>	to like,	<i>narpartolege.</i>
until soon, or then,*	<i>yopá-machuna.</i>	to lie down,	<i>meke.</i>
not yet,	<i>yopagua.</i>	to lie,	<i>kákautse.</i>
without,	<i>egárope.</i>	to laugh,	<i>idle.</i>
yes,	<i>éye.</i>	to look,	<i>take.</i>
no,	<i>chuli.</i>	to lose,	<i>tougue.</i>
and,	<i>caca</i> (?)	to make,	<i>sope</i> (cf. to build).
to braid,	<i>oyage.</i>	to open,	<i>écae.</i> ,
to break,	<i>piske.</i>	to push,	<i>épike.</i>

\* In salutation or leave taking.

to pound,	<i>sirsoe.</i>	to wind (?)	<i>púrwaga.</i> (doubtful.)
to paint,	<i>make</i> (? See 'shoot').	to walk (or go),	<i>nae.</i>
to put on clothes,	<i>mor'yoé.</i>	to wake (intr.),	<i>atage.</i>
to rise,	<i>nakwe</i> (cf. to climb).	to wash,	<i>énuke.</i>
to rub,	<i>élige.</i>		
to return,	<i>nonicowe</i> , or <i>nolicowe</i> (to come back).		<b>NUMERALS.</b>
to speak,	<i>chunmake.</i>	1. <i>quinchiqua.</i>	6. <i>nérqua.</i>
to steal,	<i>atarsáe.</i>	2. <i>póguia.</i>	7. <i>kigüle</i> (ü very short.)
to squeeze,	<i>kuwe.</i>	3. <i>páguia.</i>	8. <i>paabünca.</i>
to smoke,	<i>pib-we</i> , or <i>chigali-we</i> .*	4. <i>pakégua.</i>	9. <i>pake-pake.</i>
to sit down,	<i>chigue.</i>	5. <i>atala.</i>	10. <i>anbegi.</i>
to stand,	<i>iuisqua.</i>	11. <i>anbegi-coco-quinchiqua.</i>	
to sleep,	<i>cápe.</i>	12. " " <i>póguia.</i>	
to sink,	<i>toge.</i>	13. " " <i>páguia.</i>	
to shut,	<i>etige.</i>	14. " " <i>pakégua.</i>	
to sell,	<i>uke.</i> (cf. to take.)	15. " " <i>atala.</i>	
to swim,	<i>obe.</i>	16. " " <i>nérqua.</i>	
to shoot,	<i>make.</i>	17. " " <i>kugüla.</i>	
to see,	<i>legsaguá.</i>	19. " " <i>pake-pake.</i>	
to sing,	<i>námake.</i>	20. <i>tula-guena.†</i>	
to shake hands,	<i>arkankae.</i>	30. <i>tula-guena coca anbegi.</i>	
to take,	<i>uke.</i> (cf. to sell, to give.)	35. <i>tula-guena coca anbegi coca atala.</i>	
to throw,	<i>miye.</i> (cf. to dig).	40. <i>tula-póguia.</i>	
to tie up, or bind,	<i>estine.</i>	50. <i>tula-póguia coca anbegi.</i>	
to turn,	<i>árpabána.</i>	60. <i>tula-páguia.</i>	
to tattoo,	<i>namúrkale.</i>	70. <i>tula páguia coca unbegi.</i>	
to try,	<i>apiarléke.</i>	80. <i>tula-pakeguá.</i>	
to touch,	<i>piläke.</i>	90. <i>tula-pakeguá coca anbegi.</i>	
to tell,	<i>soje.</i>	100. <i>tula-atálu.</i>	
to wait,	<i>ablake.</i>	120. <i>tula-nérqua.</i>	
to wish for,	<i>apeye.</i>	200. <i>tula'tala-ila-póguia.</i>	
		1000. <i>tula wala guena.</i>	

How do you do ?

Good-bye,  
What is your name ?  
My name is \_\_\_\_\_,  
Where is your village ?  
Where are you going ?  
I want a big bunch of plantains,  
Have you bananas ?  
Ripe bananas,  
Do you want bananas ?  
I have meat,

*Nuerdi.* ('Good.' The salutation on  
meeting.)

*Nadroge.*  
*Igi per nuga ?*  
*Ani nuga \_\_\_\_\_*  
*Pia pe'necuebro ?*  
*Pia pe'nae ?*  
*An'apeye machi sala tumadi.*  
*Pe'womáduno nica ?*  
*Wamaduno quartigeti.*  
*Wamaduno pé'ape ?*  
*Ani sana nica.*

\*[Is this from Sp. *cigarro*?]

†[*Tulu-guena* 'one man.' Forty, *tulü-póguia*, 'two men.' ]

Give me this,	<i>An'kaugtage.</i>
I give you,	<i>Ani pe's-uke.</i>
I pay you,	<i>Ani pe'n-uke.</i>
You can [may?] go,	<i>Pe'pinche nae.</i>
I see your house,	<i>Ani pe'neka take.</i>
You have seen my house,	<i>Pe an'neka tekisa.</i>
Indian has no beard,	<i>Indio tziyu chuli.</i>
We are going,	<i>Na'mala.</i>
Go to bed,	<i>Koku'nae.</i>
I slept well,	<i>Ani nuerdi enkapisu.</i>
Very near,	<i>Ita kago.</i>
Very far,	<i>Tica chuli.</i>









